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PROCEEDINGS

OF THE

BIOLOGICAL SOCIETY OF WASHINGTON.

PUBLISHED WITH THE CO-OPERATION OF THE SMITHSONIAN INSTITUTION.

VOLUME IV.

FEBRUARY 20, 1886, TO JANUARY 28, 1888.

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BIOLOGICAL SOCIETY OF WASHINGTON.

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* Ex-Presidents of the Society.

LIST OF THE OFFICERS AND COUNCIL

OF THE

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ELECTED JANUARY 14, 1888.

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Committee on Communications.

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C. HART MERRIAM,
FREDERIC A. LUCAS,

Committee on Publications.

C. HART MERRIAM, *Chairman.*

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F. LAMSON SCRIBNER.

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* Ex-Presidents of the Society.

JOINT COMMISSION.

A temporary joint committee, appointed for the purpose of considering the advisability of forming a permanent joint committee, submitted the following report to each of the five societies concerned :—

WHEREAS, There now exist in Washington several scientific societies, organized with similar aims, working by similar methods, composed largely of the same members, and meeting in the same place ; and

Whereas, Matters of common interest are numerous and constantly increasing : therefore it is

Resolved, That it is the sense of this committee, that it is advisable to form a Joint Commission of the Anthropological, Biological, Chemical, Geographic and Philosophical Societies of Washington to consider questions of common interest ;

That such Joint Commission shall consist of three representatives from each of the five Societies ;

That its functions shall be advisory, except that it may execute instructions on general subjects, and in special cases, from two or more of the Societies participating ;

Provided, That no Society shall be bound by the Commission to an act as to which it has not given instruction.

The above resolution resulted in the establishment of a permanent Joint Commission, composed of the following delegates :

Anthropological Society.

ROBERT FLETCHER,
WASHINGTON MATTHEWS,
F. A. SEELY.

Chemical Society.

J. H. KIDDER,
F. W. CLARK,
H. W. WILEY.

Biological Society.

WILLIAM H. DALL,
C. HART MERRIAM,
RICHARD RATHBUN.

National Geographic Society.

GARDNER HUBBARD,
HENRY GANNETT,
JOHN R. BARTLETT.

Philosophical Society.

GARRICK MALLERY,
J. W. POWELL,
MARCUS BAKER.

PROCEEDINGS.[*]

NINETIETH MEETING, February 20, 1886.

The President in the chair, and thirty-seven persons present.

Dr. D. E. Salmon and Dr. Theobald Smith presented a paper, which was read by the latter, entitled, ON A NEW METHOD OF PRODUCING IMMUNITY FROM CONTAGIOUS DISEASES.

A paper by Prof. C. V. Riley, describing A CARNIVOROUS BUTTERFLY LARVA, FENESICA TARQUINIUS,[†] was read by Mr. J. B. Smith. Specimens of both the larva and imago were exhibited.

Prof. L. F. Ward spoke upon THE PLANE TREE AND ITS ANCESTORS,[‡] and exhibited specimens and figures of both the recent and fossil species.

Dr. C. Hart Merriam described A NEW SPECIES OF APLODONTIA FROM CALIFORNIA,[§] and exhibited skins and skulls of the only two species of the genus at present known.

NINETY-FIRST MEETING, March 6, 1886.

The President in the chair, and thirty-six persons present.

Dr. George Vasey spoke upon NEW AND RECENT SPECIES OF NORTH AMERICAN GRASSES.

Mr. Charles Hallock read a paper entitled HYPER-INSTINCT IN ANIMALS.

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[*] Until March 19, 1887, the meetings were held either in the Lecture Room or in the office of the National Museum, and subsequently in the Assembly Hall of the Cosmos Club, on Lafayette Square.

[†] 1886. Amer. Nat., June; and Proc. Ent. Soc., Washington, i, No. 2, p. 37.

[‡] The Paleontological History of the Genus Platanus. <Proc. U. S. Nat. Mus., xi. (In course of publication.)

[§] 1886. MERRIAM, C. HART. *Description of a New Species of Aplodontia from California.* < Ann. N. Y. Acad. Sci., iii, No. 10, pp. 312–328, plates 19, 20, and two tables.

NINETY-SECOND MEETING, March 20, 1886.

The President in the chair, and twenty-one persons present.

The following communications were presented:

Dr. D. E. Salmon and Dr. Theobald Smith, NOTES ON SOME BIOLOGICAL ANALYSES OF POTOMAC DRINKING WATER.

Dr. H. G. Beyer, REMARKS ON ANTI-PYRETICS.

Dr. W. S. Barnard, THE EFFECTS OF KEROSENE ON ANIMAL AND VEGETABLE LIFE, with exhibition of a fungus that had developed in an emulsion of kerosene and milk.

Mr. F. H. Knowlton, ADDITIONS TO, AND CHANGES IN, THE FLORA COLUMBIANA FOR 1885.*

NINETY-THIRD MEETING, April 3, 1886.

The President in the chair, and twenty-two members present.

Mr. J. B. Smith read a paper entitled SOME PECULIAR SECONDARY SEXUAL CHARACTERS IN THE DELTOIDS, AND THEIR SUPPOSED FUNCTIONS.

Dr. C. Hart Merriam described a NEW SUBSPECIES OF GRAY SQUIRREL FROM CENTRAL MINNESOTA.†

A paper by Dr. R. W. Shufeldt, on SOME EARLY, AND AS YET UNPUBLISHED, DRAWINGS OF AUDUBON, was read by Mr. F. W. True. Photographs of the drawings were exhibited.

Dr. Frank Baker and Mr. J. L. Wortman spoke upon RECENT INVESTIGATIONS INTO THE MECHANISM OF THE ELBOW JOINT.‡

NINETY-FOURTH MEETING, April 17, 1886.

Prof. Ward, Vice-President, in the chair, and seventeen persons present.

Prof. Theodore Gill described THE CHARACTERISTICS AND FAMILIES OF INIOMOUS FISHES.

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* 1886. These Proceedings, iii, pp. 106-132.

† Science, April 16, 1886, 351.

‡ Embodied in the article, "Elbow-joint," Wood's Reference Hand-book of Medical Sciences, vol. ii.

Mr. F. A. Lucas read a paper entitled NOTES ON THE VERTEBRÆ OF AMPHIUMA, SIREN, AND MENOPOMA.*

Mr. F. W. True gave an account of SOME DISTINCTIVE CRANIAL CHARACTERS OF THE CANADIAN LYNX,† with exhibition of specimens, and also exhibited a specimen of a wood hare, showing an abnormal growth of fur.

NINETY-FIFTH MEETING, May 1, 1886.

The President in the chair, and twenty-six persons present.

Prof. R. E. C. Stearns read a paper entitled INSTANCES OF THE EFFECT OF MUSICAL SOUNDS ON ANIMALS.

Mr. John A. Ryder spoke upon THE EVOLUTION OF THE MAMMALIAN PLACENTA,‡ which, he contended, had passed in its evolution from a diffuse, through a zonary, to a discoidal condition.

Mr. W. H. Dall exhibited specimens of LINGULA (GLOTTIDIA) PYRAMIDATA, Stimpson, attached to sand and bits of shell by the tip of the peduncle. He also described THE SUPERFICIAL ANATOMY OF DIFFERENT SPECIES OF THE GENUS PECTEN.§

NINETY-SIXTH MEETING, May 29, 1886.

The President in the chair, and twenty-two persons present.

Mr. J. B. Smith read a paper on ANTS' NESTS AND THEIR INHABITANTS.‖

Dr. T. H. Bean presented a communication on THE TROUT of North America, with exhibition of specimens, which was followed by a long discussion, in which many members participated.

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* 1886. LUCAS, F. A. *The Sacrum of Menopoma.* <Amer. Nat., xx, pp. 561, 562, June.

† 1887. Proc. U. S. Nat. Mus., x, pp. 8, 9.

‡ A Theory of the Origin of Placental Types, and on certain vestigiary structures on the placentæ of the mouse, rat, and field-mouse. American Naturalist, August, 1887, pp. 770-784 (with two figs.)

See also (the placentation of the two-toed ant-eater, Cycloturus didactylus), Proc. Acad. Nat. Sci., 1887, p. ——.

§ 1886. Bull. Mus. Comp. Zool., xii, No. 6.

‖ 1886. Amer. Nat., xx, pp. 679-687, August.

Prof. L. F. Ward exhibited a Specimen of the Palo La Cruz, or Wood of the Cross, obtained in Northern Brazil.

Ninety-Seventh Meeting, October 16, 1886.

The President in the chair, and twelve members present.

The Secretary read a letter from Dr. Basil Norris, U. S. A., Spokane Falls, W. T., descriptive of the larval form of a species of *Amblystoma*, probably *A. tigrina*, a specimen of which was exhibited.

Mr. F. H. Knowlton read a paper on Fasciation in Ranunculus and Rudbeckia, exhibiting specimens of each of the genera, and reviewing the different theories held by authors as to the cause of this structure. Remarks upon the same subject were made by Dr. Fernow, Prof. Ward, and Mr. Mann.

Mr. J. B. Smith gave an account of an abnormal abundance of Dynastes tityus, one of the largest of the American beetles, and having an intensely disagreeeble odor. It occasionally occurs in the District of Columbia, and ranges south and west from there into Texas and Mexico.*

Mr. F. W. True presented A Revision of the Genus Lagenorhynchus. He also exhibited an abnormally developed hoof of a mule, which was curved and twisted like a ram's horn, and a living specimen of the *Almiqui* (*Solenodon cubanus*) from Cuba, the largest known American Insectivore.

Ninety-Eighth Meeting, October 30, 1886.

The President in the chair, and ten members present.

Prof. Theodore Gill presented a communication on Tæniosomous Fishes.†

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* 1887. Popular Science Monthly, xxx, pp. 409, 410, January.

† The Characteristics and Relations of the Ribbon-fishes. <Am. Nat., v. 21, p. 86, Jan., '87.

Dr. H. G. Beyer, U. S. N., called attention to an alleged method of instructing the memory, which is being widely advertised.

NINETY-NINTH MEETING, November 13, 1886.

The President in the chair, and twenty-two persons present.

The following amendment to the Constitution, on motion of Mr. Dall, was unanimously adopted: "No person shall be considered a member of the Society until he shall have signified to the Secretary, in writing, his acceptance of election, and shall have paid his entrance fee and annual dues for the year in which he shall have been elected."

Dr. Filip Trybom, Inspector of Fisheries, of Sweden, read a paper ON THE RECENT PROGRESS OF ZOÖLOGY IN SWEDEN.*

Prof. J. W. Chickering, Jr., under the title, TRAVELS IN ALASKA, gave a graphic description of the coast scenery of British Columbia and southeastern Alaska, as seen from the deck of a passenger steamer.

Mr. William H. Dall presented some HISTORICAL NOTES ON THE DEPARTMENT OF MOLLUSKS OF THE NATIONAL MUSEUM.†

ONE HUNDREDTH MEETING, November 27, 1886.

The President in the chair, and twenty-five persons present.

Prof. W. H. Seaman presented a communication entitled NOTES ON MARSILIA QUADRIFOLIA, illustrating his remarks with stereopticon views, and herbarium and microscopical specimens. Prof. Ward referred to the paleontological history of the order containing the *Marsilia*.

Prof. L. F. Ward spoke upon THE AUTUMNAL HUES OF THE COLUMBIAN FLORA, which he thought were much brighter and finer than farther north. This paper gave rise to a long discussion, in which Prof. Riley, Dr. Merriam, Mr. Mann, and Mr. Goode participated.

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* 1887. TRYBOM, FILIP. *The Present Condition of the Natural Sciences in Sweden.* < Amer. Nat., xxi, pp. 409–415, May.

† Annual Rept. U. S. Nat. Mus. for 1886.

Dr. C. Hart Merriam described A NEW SPECIES OF BAT, VESPERTILIO CILIOLABRUM, from the Western States.*

ONE HUNDRED AND FIRST MEETING, December 11, 1886.

The President in the chair, and twenty-three persons present.

The following papers were read:

Dr. Theobald Smith, PARASITIC BACTERIA AND THEIR RELATION TO SAPROPHYTES.

Mr. F. A. Lucas, ON THE OSTEOLOGY OF THE SPOTTED TINAMOU, NOTHURA MACULOSA.†

Mr. C. D. Walcott, CRUSTACEAN TRACKS FOUND ON STRATA OF UPPER CAMBRIAN (POTSDAM) AGE.

Dr. Frank Baker, THE FORAMEN OF MAGENDIE.‡

Dr. C. Hart Merriam, DESCRIPTION OF A NEW SUB-SPECIES OF POCKET GOPHER, FROM THE COLORADO DESERT OF SOUTHERN CALIFORNIA.§

ONE HUNDRED AND SECOND MEETING, January 8, 1887.

(Seventh Annual Meeting.)

The President in the chair, and twenty-one members present.

The annual reports of the Secretary and Treasurer were read and accepted.

The following board of officers was elected for the ensuing year:

President—Mr. William H. Dall.

Vice-Presidents—Prof. Lester F. Ward, Dr. Frank Baker, Mr. C. D. Walcott, Dr. C. Hart Merriam.

Secretaries—Mr. Richard Rathbun, Mr. Frederic A. Lucas.

Treasurer—Mr. F. H. Knowlton.

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* 1886. These Proceedings, iv, pp. 1–4 (Extras issued Dec. 17, 1886).

† 1886. Proc. U. S. Nat. Mus., p. 157.

‡ Embodied in the article, "Meninges," Wood's Reference Hand-book of Medical Sciences, vol. viii.

§ Science, Dec. 24, 1886, 588.

Additional Members of the Council—Dr. T. H. Bean, Dr. George Vasey, Prof. O. T. Mason, Dr. H. G. Beyer, Prof. R. E. C. Stearns.

ONE HUNDRED AND THIRD MEETING, January 22, 1887.

(Seventh Anniversary Meeting.)

The President, Mr. Dall, occupied the chair, and about seventy-five persons were present, including invited guests.

The retiring President, Mr. G. Brown Goode, delivered an address, entitled, THE BEGINNINGS OF NATURAL HISTORY IN AMERICA—THE THIRD CENTURY.*

ONE HUNDRED AND FOURTH MEETING, February 5, 1887.

The President occupied the chair, and thirty-five persons were present, including Mr. Alfred Russel Wallace, of England.

Mr. William T. Hornaday read a paper entitled THE LAST OF THE BUFFALO, in which he described the rapid destruction of this species, and narrated his recent experiences in obtaining specimens for the National Museum.

Prof. Cope, Dr. Merriam, and Mr. Fernow made remarks upon the same subject.

Mr. Richard Rathbun exhibited a series of temperature charts prepared by the U. S. Fish Commission to illustrate the surface water temperatures of the Atlantic sea coast of the United States, in connection with the migrations of fishes.†

Mr. Dall spoke upon the value of temperature observations in studying the distribution of marine animals.

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* These Proceedings, pp. 9-94. Extras printed with cover and title page.

† 1887. RATHBUN, RICHARD. *Ocean Temperatures of the Eastern Coast of the United States, from observations made at twenty-four light-houses and light-ships.* <U. S. Commission of Fish and Fisheries. * * * The Fisheries and Fishery Industries of the United States. * * * By George Brown Goode * * * and a Staff of Associates, Section iii, pp. 155-176, 32 folding plates, quarto.

Dr. C. Hart Merriam described A NEW SPECIES OF WOOD RAT, NEOTOMA BRYANTI, FROM CERROS ISLAND, off Lower California.*

Mr. Leonhard Stejneger exhibited specimens of several NEW SPECIES OF BIRDS FROM THE SANDWICH ISLANDS,† and made remarks upon the avifauna of that region.

Mr. Eaduard Muybridge, of Philadelphia, by invitation, exhibited a series of his photographic views of animals in motion, and explained the process of taking them. The assistance of these views in explaining some obscure points in the evolution of vertebrates was pointed out by Prof. Cope.

ONE HUNDRED AND FIFTH MEETING, February 19, 1887.

Prof. Ward, Vice-President, in the chair, and twenty-two persons present.

The presiding officer announced that an invitation had been received from the Cosmos Club to use its new hall for the future meetings of the Society. It was accepted.

Prof. E. D. Cope described A NEW SPECIES OF SNAKE, from the District of Columbia, closely related to the common Water Snake, *Tropidonotus sipedon*, which he proposes to call *T. bisectus*.‡ He also spoke upon THE HYOID APPARATUS IN THE URODELE BATRACHIANS.

Dr. George Vasey made some remarks upon A RECENT COLLECTION OF MEXICAN GRASSES, OBTAINED BY DR. E. PALMER, and exhibited specimens of the rarer species.

Prof. R. E. C. Stearns read a paper on THE ASCLEPIAD PLANT, ARAUJIA ALBANS,§ and explained the mechanism of its blossoms in capturing Lepidoptera. This subject was further discussed by Prof. Riley, Mr. Smith, Prof. Ward, Dr. Baker, and Prof. Cope.

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* 1887. Amer. Nat., xxi, No. 2, pp. 191–193.

† 1887. STEJNEGER, LEONHARD. *Birds of Kauai Island, Hawaiian Archipelago, collected by Mr. Valdemar Knudsen, with descriptions of new species.* <Proc. U. S. Nat. Mus., x, pp. 75–102.

‡ 1887. Proc. U. S. Nat. Mus., x, p. 146.

§ 1887. STEARNS, R. E. C. *Araujia albens as a moth trap.* <Am. Nat., xxi, pp. 501–507.

ONE HUNDRED AND SIXTH MEETING, March 5, 1887.

Prof. Ward, Vice-President, in the chair, and twenty-eight persons present.

Mr. P. L. Jouy presented a communication entitled COREA; THE COUNTRY AND THE PEOPLE, and exhibited a large series of native implements and utensils, and also many photographs.

Dr. Frank Baker described SOME UNUSUAL MUSCULAR VARIATIONS IN THE HUMAN BODY,* which had recently come under his notice, illustrating his remarks with the aid of diagrams and prepared specimens.

Dr. C. Hart Merriam exhibited and described A NEW SPECIES OF WOOD MOUSE, EVOTOMYS CANADENSIS, recently received from the mountains of North Carolina.

Dr. H. G. Beyer made some remarks upon THE PRESERVATION OF BOTTLED MUSEUM SPECIMENS, especially in the line of Materia Medica.

ONE HUNDRED AND SEVENTH MEETING, March 19, 1887.

Prof. Ward, Vice-President, in the chair, and twenty-two persons present.

Mr. L. O. Howard read a paper entitled A ROCK CREEK PHILANTHROPIST,† the philanthropist being the larva of a species of *Hydropsyche*, which preys upon the abundant larvæ of the black fly (*Simulium venustum*).

Mr. Charles Hallock described THE TRANS-CONTINENTAL RANGE OF THE MOOSE, ALCES MACHLIS, IN NORTH AMERICA.‡

Dr. T. H. Bean compared AMERICAN AND EUROPEAN WORK IN DEEP SEA ICHTHYOLOGY, much to the credit of the former country.

Mr. F. A. Lucas noted THE OCCURRENCE OF NOCTURNAL LEPIDOPTERA AT SEA, mentioning some twelve or thirteen species which had been found distant from land.§

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* Published in the New York Medical Record, December 31, 1887, vol. xxxii, No. 27, under the title, "Some Unusual Muscular Anomalies."

† 1886. Published in part in Annual Rept., Dept. of Agriculture, 1886, p. 510.

‡ 1887. American Field, xxvii, 15, 344, April 9.

§ Science, April 8, 1887.

Capt. J. W. Collins, under the title SOME NOVEL FACTS IN THE NATURAL HISTORY OF THE CODFISH, described certain curious variations in the species, and exhibited several articles found in the stomachs or imbedded in the flesh. The most peculiar of these was a small hand-made knife of curious workmanship.

Dr. C. Hart Merriam described A NEW SPECIES OF MOUSE FROM NEW MEXICO (HESPEROMYS ANTHONYI).*

ONE HUNDRED AND EIGHTH MEETING, April 2, 1887.

The Society met for the first time in the Assembly Hall of the Cosmos Club. The President occupied the chair, and thirty persons were present.

Dr. Theobald Smith described the QUANTITATIVE VARIATIONS IN THE GERM LIFE OF POTOMAC WATER DURING 1886.

Dr. Edward Eggleston made an interesting communication, in the form of queries, addressed to the members of the Society, respecting CERTAIN PLANTS AND ANIMALS KNOWN TO THE FIRST COLONISTS OF NORTH AMERICA. Many replies were obtained.

Prof. O. T. Mason exhibited and described a large series of REPRESENTATIONS OF ANIMAL LIFE IN ESKIMO ART.

Mr. F. W. True gave an account of THE BLACKFISH OF OUR SOUTHERN WATERS.

ONE HUNDRED AND NINTH MEETING, April 16, 1887.

The President in the chair, and forty-one persons present.

Mr. W. H. Dall described some RECENT GEOLOGICAL EXPLORATIONS IN SOUTHWESTERN FLORIDA,† made by himself. The observations were discussed by Mr. G. K. Gilbert and Dr. T. Sterry Hunt.

Dr. H. G. Beyer spoke upon THE ACTION OF CAFFEINE UPON THE KIDNEYS.

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* 1887. These Proceedings, iv, pp. 5-8. (Extras issued April 15, 1887.)

† 1887. DALL, WILLIAM H. *Notes on the Geology of Florida.* <Amer. Jour. Sci., xxxiv, pp. 162-170.

Dr. C. Hart Merriam read a paper detailing the RAVAGES OF THE BOBOLINK IN THE RICE FIELDS OF THE SOUTH.*

ONE HUNDRED AND TENTH MEETING, April 30, 1887.

The President in the chair, and thirty-eight persons present.

Dr. J. H. Kidder exhibited a rounded concretion-like mass taken from the stomach of a codfish; and also several rounded grass balls from a small salt pond near Pyramid Lake, Nevada, and explained their composition. These gave rise to much discussion, and Mr. McGee, who had collected the grass balls, described the manner of their formation.

Mr. F. A. Lucas spoke upon THE OS PROMINENS IN BIRDS.

Mr. W. T. Hornaday read a paper entitled CIVILIZATION AS AN EXTERMINATOR OF SAVAGE RACES, which led to some remarks by Prof. Ward and Mr. Dall.

Mr. W. H. Dall called attention to A GENUS OF BIVALVE MOLLUSKS NEW TO NORTH AMERICA. The genus is *Cyrenella*.†

ONE HUNDRED AND ELEVENTH MEETING, May 14, 1887.

The President in the chair, and forty-two persons present.

Prof. C. V. Riley presented some BIOLOGICAL NOTES ON SOUTHERN CALIFORNIA, suggested by a recent trip to that region. Remarks were made by Dr. Vasey, Dr. Merriam, Prof. Stearns, and Mr. Dall.

Mr. P. L. Jouy exhibited specimens of A BIRD NEW TO JAPAN, PITTA OREAS OF SWINHOE, from the island of Tsushima.

Mr. F. H. Knowlton made a communication on THE RECENT SHOWER OF POLLEN IN WASHINGTON, the so-called "sulphur shower." The distance which pollen may be carried by the winds gave rise to remarks by Dr. Vasey, Prof. Riley, and Prof. Ward.

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* 1887. Published in part in Annual Rept. Dept. of Agriculture for 1886, pp. 246–250.

† 1887. Amer. Jour. Sci., xxxiv, p. 170.

The question, "DOES THE FLYING FISH FLY?" was discussed by Mr. W. B. Barrows, Engineer G. W. Baird, U. S. N., Mr. Lucas, Mr. Goode, Mr. Hallock, Mr. Dall, and Prof. Riley.

ONE HUNDRED AND TWELFTH MEETING, May 28, 1887.

The President in the chair, and twenty-one persons present.

Prof. R. E. C. Stearns read a paper entitled THE PROTECTIVE DEVICES IN THE "CARRIER SHELL," XENOPHORA, and exhibited specimens of several species.

Mr. R. T. Hill explained THE TRUE GEOLOGICAL HORIZON OF SOME HITHERTO UNPLACED FAUNAS, with special reference to the Cretaceous of Texas. Mr. McGee made some remarks on Mr. Hill's paper.

Mr. G. Brown Goode exhibited a series of JAPANESE CHROMO-LITHOGRAPHS OF FISHES, recently published. Mr. Baba, of Japan, spoke upon Japanese methods of delineation, and the subject was further discussed by Prof. Gill, Prof. Riley, Mr. Dall, Mr. Stejneger, and Prof. Seaman.

ONE HUNDRED AND THIRTEENTH MEETING, October 22, 1887.

The President in the chair, and forty persons present.

The President announced the death, during the summer recess, of Prof. Spencer F. Baird, the only honorary member of the Society, and of Dr. Charles Rau, one of its most distinguished active members.

Mr. L. O. Howard described AN ANT-DECAPITATING PARASITE, the larva of a species of Diptera, probably belonging to the family *Conopidæ*, from New Hampshire.

Dr. George Vasey presented some NOTES ON WESTERN GRASSES.

Mr. F. A. Lucas read a paper entitled THE BIRD ROCKS OF THE GULF OF SAINT LAWRENCE IN 1887.* These rocks are situated in the Gulf of St. Lawrence, and were visited, during the summer of 1887, by Mr. Lucas with the Fish Commission schooner *Grampus*.

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* 1888. The Auk, April.

Mr. A. A. Crozier, under the title, Some Botanical Terms, referred to the ambiguity attending the use of the words "sinistrorse" and "dextrorse," as applied to twining plants.

Dr. C. Hart Merriam gave an account of the Fauna and Flora of the Great Smoky Mountains in North Carolina and Tennessee.

One Hundred and Fourteenth Meeting, November 5, 1887.

The President in the chair, and thirty-six persons present.

Mr. John B. Smith read a paper on Some Geographical Variations of Insects, with special reference to local variations in Lepidoptera and Coleoptera.

Dr. T. H. Bean presented a communication respecting The Young Forms of Some of Our Food Fishes, and exhibited alcoholic specimens of the same.

Mr. N. P. Scudder explained The Period of Gestation in the Common Caged White Mouse.

Mr. H. E. Van Diemen exhibited specimens of the fruit and colored drawings of the foliage, flowers, and fruit of The Japanese Persimmon, Diospyros kaki.

Prof. Theodore Gill described the characteristics of The Fish Fauna of the South Temperate or Notalian Realm.

One Hundred and Fifteenth Meeting, Nov. 19, 1887.

Prof. Ward, Vice-President, in the chair, and thirty-two persons present.

Col. Marshall McDonald presented an Explanation of Past Failures in the Culture of the Salmonidæ.

Mr. Walter B. Barrows read a paper entitled Freshet Notes on the Rio Uruguay, South America.

Dr. T. H. Bean described A New Species of Thyrsitops

FROM THE NEW ENGLAND FISHING BANKS,* with the aid of photographs and a life-size crayon sketch.

Mr. F. W. True gave a review of some of the more important works on Cetaceans published since 1886.

Mr. F. A. Lucas read a paper entitled AN ALCINE CEMETERY, being the resting-place of the Great Auk on Funk Island, off Newfoundland.

Mr. H. E. Van Diemen called attention to a cluster of the fruit of the date palm, *Phœnix dactylifera*, from New Orleans, which he had placed upon the table for examination.

ONE HUNDRED AND SIXTEENTH MEETING, December 3, 1887.

The President in the chair, and thirty-nine persons present.

Mr. Charles Hallock read a paper descriptive of THE GREAT ROSEAU SWAMP of northwestern Minnesota.

A communication from Dr. C. A. White, on THE RAPID DISAPPEARANCE OF THE SHED ANTLERS OF THE CERVIDÆ, was read by the Secretary.

Dr. Theobald Smith made a few remarks upon PEPTONIZING FERMENTS AMONG BACTERIA.

Mr. C. D. Walcott exhibited A FOSSIL LINGULA PRESERVING THE CAST OF THE PEDUNCLE, from the Hudson Terrane, near Rome, N. Y.

Prof. Theodore Gill discussed THE PHYLOGENY OF THE CETACEA.

ONE HUNDRED AND SEVENTEENTH MEETING, Dec. 17, 1887.

Dr. C. Hart Merriam, Vice-President, in the chair, and twenty-three persons present.

Mr. C. L. Hopkins read a paper entitled NOTES RELATIVE TO THE SENSE OF SMELL IN THE TURKEY BUZZARD.

Dr. Cooper Curtice described some recent observations respecting THE TIMBER LINE OF PIKE'S PEAK.

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* Proc. U. S. Nat. Mus., x (in course of publication).

Mr. C. D. Walcott exhibited a SECTION OF A FOSSIL ENDOCERAS OVER EIGHT FEET IN LENGTH, and explained its structure and relations to other shell-bearing Cephalopoda, both fossil and recent.

Mr. Leonhard Stejneger read a paper entitled HOW THE GREAT NORTHERN SEA COW, RHYTINA, BECAME EXTERMINATED.*

ONE HUNDRED AND EIGHTEENTH MEETING, Dec. 31, 1887.

The President occupied the chair, and sixteen persons were present.

Mr. W. J. McGee spoke upon THE OVER-LAPPING HABITATS OF STURNELLA MAGNA AND STURNELLA NEGLECTA, IN IOWA.

Dr. C. Hart Merriam exhibited and described A NEW SPECIES OF FIELD MOUSE, ARVICOLA (CHILOTUS) PALLIDUS, FROM THE BAD LANDS OF NORTHWESTERN DAKOTA.

Mr. W. B. Barrows described THE SHAPE OF THE BILL IN SNAIL-EATING BIRDS, with special reference to the Kite, *Rostrhamus sociabilis*, and the "crying" birds, *Aramus*.

A paper by Mr. H. Justin Roddy, on the FEEDING HABITS OF SOME YOUNG RAPTORES, was read by Mr. Lucas.

ONE HUNDRED AND NINETEENTH MEETING, Jan. 14, 1888.

(Eighth Annual Meeting).

The President occupied the chair, and twenty-seven members were present.

The annual reports of the Secretary and Treasurer were read and accepted.

The following board of officers was elected for the ensuing year:

President—Mr. William H. Dall.

Vice-Presidents—Dr. C. Hart Merriam, Prof. L. F. Ward, Prof. C. V. Riley, Mr. Richard Rathbun.

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* 1887. American Naturalist, xxi, pp. 1047–1054, December.

Secretaries—Mr. J. B. Smith, Mr. F. A. Lucas.

Treasurer—Mr. F. H. Knowlton.

Additional Members of the Council—Dr. T. H. Bean, Dr. J. H. Kidder, Prof. R. E. C. Stearns, Mr. F. W. True, Dr. George Vasey.

The President announced the following Committee on Saturday Lectures: Prof. G. Brown Goode, Chairman; Dr. Frank Baker, Mr. G. K. Gilbert, Dr. C. Hart Merriam, Prof. C. V. Riley.

ONE HUNDRED AND TWENTIETH MEETING, Jan. 28, 1888.

(Eighth Anniversary Meeting).

The eighth anniversary meeting of the Society was held in the lecture hall of Columbian University, on the evening of January 28, about seventy-five persons being present.

The President, Mr. William H. Dall, delivered an address, entitled, SOME AMERICAN CONCHOLOGISTS.*

SATURDAY LECTURES, 1886.

The fifth course of Saturday Lectures under the auspices of the Biological Society and the Anthropological Society was begun March 6, 1886. The lectures were delivered in the lecture room of the National Museum, and the following programme was carried out:

March 6: Mr. WILLIAM HALLOCK. The Geysers of the Yellowstone.
March 12: Prof. WILLIAM HARKNESS. How the Solar System is measured.
March 20: Prof. T. C. MENDENHALL. The Nature of Sound.
March 27: Prof. F. W. CLARKE. The Chemistry of Coal.
April 3: Dr. C. HART MERRIAM. The Migration of Birds.
April 10: Dr. WASHINGTON MATTHEWS. The Gods of the Navajos.
April 16: Dr. D. B. SIMMONS. Social Status of the Women of Japan.
April 24: Prof. W. K. BROOKES. Life.
May 1: Prof. LESTER F. WARD. Heredity and Opportunity.
May 8: Dr. JOHN S. BILLINGS. Animal Heat.

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* These Proceedings, pp. 95–134. Extras printed with title page and cover.

SATURDAY LECTURES, 1887.

The sixth course of Saturday Lectures was begun March 12, 1887, under the auspices of the Biological, Philosophical, and Anthropological Societies. The lectures were delivered in the lecture hall of the National Museum, eight being given on Saturday afternoons, and four on Wednesday evenings with the aid of the stereopticon. The programme was as follows:

March 12: Gen. A. W. GREELY, U. S. A. Animals of the Arctic Region.

March 19: Capt. C. E. DUTTON, U. S. A. Earthquakes.

March 23: Mr. W. J. MCGEE. The Charleston Earthquake. (Evening lecture.)

March 26: Prof. OTIS T. MASON. The Natural History of Human Arts.

April 2: Dr. B. E. FERNOW. Our Forestry Problem.

April 6: Mr. THOMAS WILSON. Pre-historic Man in Europe. (Evening lecture.)

April 16: Dr. EDWARD M. HARTWELL. The Aims and Effects of Physical Training.

April 20: Dr. FRANK BAKER. Facial Expression. (Evening lecture.)

April 23: Miss H. C. DES. ABBOTT. The Chemistry of the Higher and Lower Plants.

April 30: Prof. HARRISON ALLEN. Rights and Lefts.

May 4: Prof. S. P. LANGLEY. Sunlight and the Earth's Atmosphere. (Evening lecture.)

May 7: Dr. J. H. BRYAN. The Mechanism of the Human Voice.

BAIRD MEMORIAL MEETING.

January 11, 1888, a meeting commemorative of the life and scientific work of Prof. Spencer Fullerton Baird was held in the lecture hall of the Columbian University, under the joint auspices of the Anthropological, Biological, and Philosophical Societies of Washington. A very large number of persons was in attendance. Mr. Garrick Mallery, President of the Philosophical Society, presided, and the following addresses were delivered:

RELATIONS BETWEEN PROFESSOR BAIRD AND THE PARTICIPATING SOCIETIES, by Mr. Garrick Mallery.

PROFESSOR BAIRD AS ADMINISTRATOR, by Mr. William B. Taylor, of the Smithsonian Institution.

PROFESSOR BAIRD IN SCIENCE, by Mr. William H. Dall, President of the Biological Society.

THE PERSONAL CHARACTERISTICS OF PROFESSOR BAIRD, by Mr. J. W. Powell, President of the Anthropological Society.*

BOTANICAL SECTION.

A preliminary meeting of persons interested in Botany took place November 21, 1887, in the office of the Botanist of the Department of Agriculture. A second meeting was held December 5, at which a Botanical Section of the Biological Society was formally organized. Dr. George Vasey was elected President, and Mr. A. A. Crozier, Secretary. The first regular meeting was held January 4, 1888, when the following papers were read:

1. RECENT PROGRESS IN THE STUDY OF FRESH-WATER ALGÆ, Prof. E. A. Burgess.
2. A CASE OF SEWER OBSTRUCTION BY TREE ROOTS, Prof. F. H. Knowlton.
3. FUNGI OF THE ARID REGIONS, Prof. S. M. Tracy.
4. GLŒOSPORIUM OF THE WAX BEAN, Miss E. A. Southworth.

The Section is to meet monthly.

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* These addresses, together with a portrait of Professor Baird, have been printed in the Bulletin of the Philosophical Society, vol. x, pp. 41–77, 1888. Also separately issued with independent pagination.

DESCRIPTION OF A NEW SPECIES OF BAT FROM THE WESTERN UNITED STATES.

(*Vespertilio ciliolabrum* sp. nov.)

By Dr. C. Hart Merriam.

(Read November 27, 1886.)

Specimens of a small and apparently hitherto undescribed species of bat have reached me from two widely separated localities in the Western United States. The first were collected by Mr. A. B. Baker in Trego County, Kansas; the second by Mr. A. W. Anthony in Grant County, in the extreme southwestern corner of New Mexico.

Mr. Baker writes me that "the first two of these bats were found in bluffs or cañons near the town of Banner, and were hidden away in clefts in the chalk rock. The others were captured at a bluff several miles distant. They had secreted themselves in abandoned swallows' nests which were inaccessible; but the bats were easily dislodged by means of stones. They were followed to their various places of refuge, and seven were secured."

These bats belong to the group of American *Vespertilios*, of which *V. nitidus* may be considered fairly typical. They differ from *V. nitidus*, however, in size, proportions, and color, as well as in the much larger size of the ear.

The Kansas specimens vary in color from nearly pure white to pale yellowish-brown, or even isabella-brown, while those from New Mexico are tawny-isabella above and much paler underneath.

The following characters will serve to distinguish the species from its allies:

VESPERTILIO CILIOLABRUM* sp. nov.

(Type No. 2797 female ad., Merriam Collection).

Dental formula: $$\text{i. } \frac{2-2,}{6} \text{ c. } \frac{1-1,}{1-1} \text{ pm. } \frac{3-3,}{3-3} \text{ m. } \frac{3-3,}{3-3} = \frac{18}{20} = 38.$$

The outer upper incisor of each side slopes forward and inward parallel to the inner, contrary to the rule in the genus *Vespertilio*, in which these teeth usually are divergent; cusp of inner upper incisor bifid, the anterior point being larger. First upper premolar small and crowded against (and usually somewhat internal to) the canine; second upper premolar minute and wholly internal to the tooth-row so that it is not visible from the outside except in immature individuals; third premolar very large, nearly or quite equal to canine. Middle lower premolar smallest; posterior largest.

Sides of upper lip fimbriate. Glandular prominences between eyes and nostrils moderately developed. Tip of ear laid forward extends to end of muzzle.

The calcaneum reaches about half-way from the foot to the tip of the tail; the postcalcaneal lobule is large for a *Vespertilio;* the calcaneum ends in a projecting tooth or lobule.

The form of the ear is somewhat intermediate between that of *V. nitidus* and that of *V. nigricans:* Internal basal lobe slightly rounded; middle three-fourths of anterior margin strongly convex; tip shortly rounded off, forming a small, projecting lobe posteriorly, beneath which the outer border is sharply emarginated for about one-third of its entire length; bottom of emargination straight or slightly convex; below this

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*The specific name *ciliolabrum* refers to the fringe of hairs along the sides of the upper lip.

the outer margin becomes abruptly convex and then nearly straight, with a distinct reflexed lobe near its base. Tragus attenuated above; inner margin straight or slightly convex; outer margin slightly concave in upper half, then slightly convex, with a distinct lobule at the base, which is separated by a notch from the convexity above.

Thumb very small, considerably shorter than the foot. Foot small. Wings from base of toes. Upper surface of wing-membranes haired from about the middle of the humerus to the knee; basal third of upper surface of interfemoral membrane covered with hair; on under surface of interfemoral the hair is arranged in little tufts along transverse lines, about thirteen in number. Half of last vertebra of tail free.

Fur long and soft; basal portion dusky; apical portion varying from whitish or yellowish-white to isabella-brown (tawny-isabella in the New Mexico specimens), which in some individuals is nearly as dark as in *V. subulatus;* the colored apical portion varies in extent from less than one-third to more than one-half the length of the hairs.

Measurements from alcoholic specimens.—Male adult (No. 2794 Merriam Collection): Head and body, 42 mm.; head, 16.25 mm.; tail, 37 mm.; ear, from inner basal angle, 15 mm.; tragus, 6.75 mm.; humerus, 22 mm.; forearm, 32.50 mm.; thumb, 3.75 mm.; third finger, 56 mm.; fifth finger, 44 mm.; tibia, 11.25 mm.; hind foot, 7 mm.

Female adult (type, No. 2797 Merriam Collection): Head and body, 43 mm.; head, 16.25 mm.; tail, 40 mm.; ear, from inner basal angle, 15 mm.; tragus, 6.75 mm.; humerus, 22 mm.; forearm, 33 mm.; thumb, 3.50 mm.; third finger, 56 mm.; fifth finger, 45.50 mm.; tibia, 11.50 mm.; hind foot, 7.50 mm.

Measurements of nine alcoholic specimens of Vespertilio ciliolabrum.

(All measurements are in millimetres).

No., Mus. C. H. M.	Locality.	Sex.	Head and body.	Tail.	Head.	Ear from internal basal angle.	Tragus from inner base.	Humerus.	Forearm.	Thumb.	3d finger.	5th finger.	Tibia.	Hind foot.	Collector.	Date.
2792	Trego Co., Kan.	♂ ad.	42.	35.	16.50	15.	6.75	22.50	33.	3.75	56.	44.	11.50	7.	A. B. Baker	August, 1885
2793	"	♂	42.	39.	16.	15.	6.	21.50	32.50	3.50	52.50	41.50	11.25	6.	"	" "
2794	"	♂	42.	37.	16.25	15.	6.75	22.	32.50	3.75	56.	44.	11.25	7.	"	" "
2795	"	♀	43.50	40.	16.50	15.	6.50	22.50	33.	3.25	56.	44.50	11.50	7.25	"	" "
2796	"	♀	43.50	42.	16.50	15.50	6.75	22.50	33.	3.50	57.	44.50	11.50	7.	"	" "
2797	"	♀	43.	40.	16.25	15.	6.75	22.	33.	3.50	56.	45.50	11.50	7.50	"	" "
2798	"	♀	42.	39.	16.	15.	6.25	21.50	32.	3.25	54.50	42.	11.50	6.50	"	" "
2786	Grant Co., N. M.	♀ ad.	41.	38.	16.	14.50	6.50	21.	33.	3.50	55.	43.50	11.50	6.50	A. W. Anthony	Aug. 25, 1886
2787	"	♂ im.	....	41.	15.	14.50	6.	20.	32.	3.	56.	42.	11.	7.	"	April 6, "

DESCRIPTION OF A NEW MOUSE FROM NEW MEXICO.

Hesperomys (*Vesperimus*) *Anthonyi* sp. nov.

By Dr. C. Hart Merriam.

(Read March 19, 1887).

During the spring and summer of 1886, Mr. A. W. Anthony, of Denver, Colorado, made his headquarters at Camp Apache, Grant county, New Mexico (about lat. 31° 20′). Camp Apache is in a hot desert region in the extreme southwestern corner of the Territory, and only about four miles from the Mexican boundary.

The following extract from one of Mr. Anthony's letters sufficiently describes the region. He writes: "You can form some idea of my location when I tell you that our nearest water is a very small spring nine miles across the valley, from which all our water is carried in wagons. The only trees within forty miles are a few very small stunted cedars and oaks. The only other vegetation consists of cacti and other plants characteristic of these hot dry deserts."

While in this region Mr. Anthony made a valuable collection of mammals, which he has very kindly presented to me. Among other things of interest it contains five specimens of a pretty little mouse, hitherto unknown in the United States, which I believe to be undescribed, and which, therefore, I take pleasure in dedicating to its discoverer. In coloration, proportions, and cranial characters this mouse differs so radically from all previously known species, that comparison with others is unnecessary. Unfortunately, nothing is known of its habits. It may be distinguished from its congeners by the following diagnosis:

HESPEROMYS (VESPERIMUS) ANTHONYI sp. nov.

Type No $\frac{2333}{2841}$, male ad., Merriam Collection.

Size, small; tail considerably longer than head and body; ears large and scant haired; whiskers long, reaching past shoulders. Soles naked, 6 tuberculate; palms 5 tuberculate; thumb armed with a blunt nail.

COLOR.—Upper parts from nose to tail, uniform clear ash-gray, more or less darkened by black-tipped hairs; sides bright buffy-fulvous; under parts white, the plumbeous basal portion of the hairs showing through on the chin and throat, which are thinly clothed with rather short hairs; belly strongly washed with salmon, which may be due to earth-staining. Pelage soft. The fur covering the breast, abdomen, and flanks is very much more dense than that of the rest of the body, from which it may be distinguished at a glance. In fact, on the sides it forms well-marked flank patches or tufts. Possibly this character may be seasonal; if not, it is very remarkable. In the young the belly is pure white, and the buffy-fulvous flank patches are not apparent.

The material at hand consists of five skins and skulls, collected in April and May. All are males. Nos. 2332 and 2335 are immature, though the latter is full grown. The skins were prepared with unusual care, and consequently afford measurements of approximate accuracy. Moreover, Mr. Anthony recorded the total length of each before skinning.

Table of Measurements of five Specimens of Hesperomys Anthonyi *collected at Camp Apache, Grant County, New Mexico, by A. W. Anthony.*

(*Measurements in millimeters*).

Skin No.	Skull No.	Sex and Age.	MEASURED IN THE FLESH.*	MEASURED FROM THE DRY SKIN.						
			Total length.	Total length.	Head and body.	Tail, to end of		Hind foot.	Height of Ear from crown.	Date.
						Vertebræ.	Hairs.			
2149	2675	♂ ad	165	144	63	80	81.5	18.5	12.	Apr. 12, 1886.
2332	2840	♂ im	162	145	62	82	83.5	18.5	11.	“ 5, “
2333	2841	♂ ad	168	145	63	81	82.5	19.5	12.	May 10, “
2334	2842	♂ ad	165	150	66	83	85.	19.5	12.	“ “ “
2335	2843	♂ im.	162	139	64	74	75.	19.	10.	“ “ “

CRANIAL CHARACTERS.—The skull, compared with that of *H. leucopus*, is short, broad, and flat. The incisor foramina reach past the anterior plane of the first molar. The nasals are short and do not extend so far posteriorly as the premaxillaries.

Excluding skull No. 2840, which is not full grown, the close agreement in cranial measurements is remarkable.

Cranial Measurements.

	No. 2840	No. 2841	No. 2842	No. 2843
	♂ im.	♂ ad.	♂ ad.	♂
Basilar length (from one of the occipital condyles to posterior edge of alveola of incisor of same side)	18.9	20.	20.3	20.4
Basilar length of Hensel (from inferior lip of foramen magnum to posterior edge of alveola of incisor)	16.5	18.	18.	18.
Greatest zygomatic breadth	12.4	12.8	12.7	12.1
Interorbital constriction	3.8	3.9	3.7	3.7
Greatest length of nasal bones	7.4	7.8	8.5	8.3
Length of upper molar series	3.6	3.8	3.8	3.8
Incisor to molar	5.4	5.6	5.6	5.7
“ “ post-palatal notch	8.8	9.5	9.5	9.5
Distance between alveolæ of upper molar series anteriorly	2.5	2.5	2.5	2.5
“ “ “ “ “ “ “ posteriorly	2.5	2.5	2.5	2.5
Foramen magnum to post-palatal notch	7.4	8.	8.	8.2
Height of cranium from inferior lip of foramen magnum	6.8	7.3	7.	7.
Fronto-palatal depth (taken at middle of molar series)	5.8	6.2	6.	5.8
Length of mandible	12.6	12.9	12.9	13.2
Length of under molariform series	3.7	3.8	3.8	4.

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* The apparent discrepancy between the total length as recorded by Mr. Anthony and that taken from the dry skin is due to the necessary stretching of the fresh specimens for measurement.

THE BEGINNINGS OF AMERICAN SCIENCE.*

THE THIRD CENTURY.

BY G. BROWN GOODE.

VIII.

In the address which it was my privilege, one year ago, to read in the presence of this Society, I attempted to trace the progress of scientific activity in America from the time of the first settlement by the English in 1585 to the end of the Revolution—a period of nearly two hundred years.

Resuming the subject, I shall now take up the consideration of the third century—from 1782 to the present time. For convenience of discussion the time is divided, approximately, into decades, while the decades naturally fall into groups of three. From 1780 to 1810, from 1810 to 1840, from 1840 to 1870, and from 1870 to the close of the century, are periods in the history of American thought, each of which seems to be marked by characteristics of its own. These must have names, and it may not be inappropriate to call the first the period of Jefferson, the second that of Silliman, and the third that of Agassiz.

The first was, of course, an extension of the period of Linnæus, the second and third were during the mental supremacy of Cuvier and Von Baer and their schools, and the fourth or present, begining in 1870, belongs to that of Darwin, the extension of whose influence to America was delayed by the tumults of the civil convulsion which began in 1861 and ended in 1865.

The "beginnings of American science" do not belong entirely to the past. Our science is still in its youth, and in the discussion of its history I shall not hesitate to refer to institutions and to tendencies which are of very recent origin.

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* Annual Presidential Address delivered at the Seventh Anniversary Meeting of the Biological Society of Washington, January 22, 1887, in the Lecture Room of the U. S. National Museum.

It is somewhat unfortunate that the account book of national progress was so thoroughly balanced in the Centennial year. It is true that the movement which resulted in the birth of our Republic first took tangible form in 1776, but the infant nation was not born until 1783, when the treaty of Paris was signed, and lay in swaddling clothes until 1789, when the Constitution was adopted by the thirteen States.

In those days our forefathers had quite enough to do in adapting their lives to the changed conditions of existence. The masses were struggling for securer positions near home, or were pushing out beyond the frontiers to find dwelling-places for themselves and their descendants. The men of education were involved in political discussions as fierce, uncandid, and unphilosophical in spirit as those which preceded the French revolution of the same period.

The master minds were absorbed in political and administrative problems, and had little time for the peaceful pursuits of science, and many of the men who were prominent in science—Franklin, Jefferson, Rush, Mitchill, Seybert, Williamson, Morgan, Clinton, Rittenhouse, Patterson, Williams, Cutler, Maclure, and others—were elected to Congress or called to other positions of official responsibility.

IX.

The literary and scientific activities of the infant nation were for many years chiefly concentrated in Philadelphia, until 1800 the federal capital and largest of American cities. Here, after the return of Franklin from France in 1785, the meetings of the American Philosophical Society were resumed. Franklin continued to be its president until his death in 1790, at the same time holding the presidency of the commonwealth of Pennsylvania, and a seat in the Constitutional Convention. The prestige of its leader doubtless gave to the Society greater prominence than its scientific objects alone would have secured.

In the reminiscences of Dr. Manasseh Cutler there is to be found an admirable picture of Franklin in 1787. As we read it we are taken back into the very presence of the philosopher and statesman, and can form a very clear appreciation of the scientific atmosphere which surrounded the scientific leaders of the post-Revolutionary period.

Dr. Cutler wrote:

"Dr. Franklin lives on Market street. His house stands up a court at some distance from the street. We found him in his garden sitting upon a grass-plot, under a large mulberry tree, with several gentlemen and two or three ladies. When Mr. Gerry introduced me he rose from his chair, took me by the hand, expressed his joy at seeing me, welcomed me to the city, and begged me to seat myself close by him. His voice was low, his countenance open, frank, and pleasing. I delivered to him my letters. After he had read them he took me again by the hand and, with the usual compliments, introduced me to the other gentlemen, who are, most of them, members of the Convention. Here we entered into a free conversation, and spent the time most agreeably until it was quite dark. The tea-table was spread under the tree, and Mrs. Bache, who is the only daughter of the Doctor and lives with him, served it to the company.

"The Doctor showed me a curiosity which he had just received and with which he was much pleased. It was a snake with two heads, preserved in a large vial. It was about ten inches long, well proportioned, the heads perfect, and united to the body about one-fourth of an inch below the extremities of the jaws. He showed me a drawing of one entirely similar, found near Lake Champlain. He spoke of the situation of this snake if it was travelling among bushes, and one head should choose to go on one side of the stem of a bush and the other head should prefer the other side, and neither head would consent to come back or give way to the other. He was then going to mention a humorous matter that had that day occurred in the Convention in consequence of his comparing the snake to America; for he seemed to forget that everything in the Convention was to be kept a profound secret. But this was suggested to him, and I was deprived of the story.

"After it was dark we went into the house, and he invited me to his library, which is likewise his study. It is a very large chamber and high-studded. The walls are covered with shelves filled with books; beside these, four large alcoves, extending two-thirds the length of the chamber, filled in the same manner. I presume this is the largest and by far the best private library in America. He showed me a glass machine for exhibiting the circulation of the blood in the arteries and veins of the human body. The circulation is exhibited by the passing of a red fluid from a reservoir into numerous capillary tubes of glass, ramified in every direction, and then returning in similar tubes to the reservoir, which was done with great velocity, and without any power acting visibly upon the fluid, and had the appearance of perpetual motion. Another great curiosity was a rolling press for taking copies of letters or other writing. A sheet of paper is completely copied in two minutes, the copy as fair as the original, and without defacing it in the smallest degree. It is an invention of his own, extremely useful in many circumstances of life. He also showed us his long artificial hand and arm for taking down and putting up books on high shelves, out of reach, and his great arm-chair, with rockers and a large fan placed over it, with which he fans himself, while he sits reading, with only a slight motion of the foot, and many other curiosities and inventions, all his own, but of lesser note. Over his mantel he has a prodigious number of medals, busts, and casts in wax or plaster of Paris, which are the effigies of the most noted characters of Europe. But what the Doctor wished especially to show me was a huge volume on botany, which indeed afforded me the greatest pleasure of any one thing in his library. It was a single volume, but so large that it was with great difficulty that he was able to raise it from a low shelf and lift it to the table; but, with that senile ambition which is common to old people (Dr. Franklin was eighty-one), he insisted on doing it himself, and would permit no one to assist him, merely to show how much strength he had remaining. It contained the whole of Linnæus's Systema Vegetabilium, with large cuts colored from nature of every plant. It was a feast to me, and the Doctor seemed to enjoy it as well as myself. We spent a couple of hours examining this volume, while the other gentlemen amused themselves with other matters. The Doctor is not a botanist, but lamented he did not in early life attend to this science. He delights in natural history, and expressed an earnest wish that I should pursue a plan I had begun, and hoped this science, so much neglected in America, would be pursued with as much ardor here as it is now in every part of Europe. I wanted, for three months at least, to have devoted myself entirely to this one volume, but, fearing lest I should become tedious to him, I shut the book, though he urged me to examine it longer.

He seemed extremely fond, through the course of the visit, of dwelling on philosophical subjects, and particularly that of natural history, while the other gentlemen were swallowed up in politics. This was a favorable circumstance to me, for almost all his conversation was addressed to me, and I was highly delighted with the extensive knowledge he appeared to possess of every subject, the brightness of his faculties, the clearness and vivacity of his mental powers, and the strength of his memory, notwithstanding his age. His manners are perfectly easy, and everything about him seems to diffuse an unrestrained freedom and happiness. He has an incessant vein of humor, accompanied with an uncommon vivacity that seems as natural and involuntary as his breathing."

To Franklin, as President of the Philosophical Society, succeeded David Rittenhouse [b. 1732, d. 1796], a man of world-wide reputation, known in his day as "*the* American Philosopher."*

He was an astronomer of repute, and his observatory built at Norriton in preparation for the transit of Venus in 1769 seems to have been the first in America. His orrery, constructed upon an original plan, was one of the wonders of the land. His most important contribution to astronomy was the introduction of the use of spider lines in the focus of transit instruments.†

He was an amateur botanist, and in 1771 made interesting physiological experiments upon the electric eel.‡

He was a Fellow of the Royal Society of London, and the first Director of the United States Mint.

Next in prominence to Franklin and Rittenhouse were doubtless the medical professors, Benjamin Rush, William Shippen, John Morgan, Adam Kuhn, Samuel Powell Griffiths, and Caspar Wistar, all men of scientific tastes, but too busy in public affairs and in medical instruction to engage deeply in research, for Philadelphia, in those days as at present, insisted that all her naturalists should be medical professors, and the active investigators, outside of medical science, were not numerous. Rush, however, was one of the earliest American writers upon ethnology, and a pathologist of the highest rank. He is generally referred to as the earliest professor of chemistry, having been appointed to the chair of chemistry in the College of Philadelphia in 1769; it seems certain, however, that Dr. John Morgan lectured on chemistry as early as 1765.*

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* See obituary in the *European Magazine*, July, 1796; also Memoirs of Rittenhouse, by WILLIAM BARTON, 1813, and Eulogium by Benjamin Rush, 1796.

† VON ZACH: Monatliche Correspondenz, ii, p. 215.

‡ Phila. Medical Repository, vol. 1.

Dr. Shippen [b. 1735, d. 1808], the founder of the first medical school [1765] and its professor of anatomy for forty-three years, was still in his prime, and so was Dr. Morgan [b. 1735, d. 1789], a Fellow of the Royal Society, a co-founder of the medical school, and a frequent contributor to the Philosophical Transactions. Morgan was an eminent pathologist, and is said to have been the one to originate the theory of the formation of pus by the secretory action of the vessels of the part.† He appears to have been the first who attempted to form a museum of anatomy, having learned the methods of preparation from the Hunters and from Süe in Paris. The beginning was still earlier known, for a collection of anatomical models in wax, obtained by Dr. Abraham Chovet in Paris, was in use by Philadelphia medical students before the Revolution.‡

Another of the physicians of colonial days who lived until after the revolution was Dr. Thomas Cadwallader [b. 1707, d. 1779], whose dissections are said to have been among the earliest made in America, and whose "Essay on the West India Dry Gripes," 1775, was one of the earliest medical treatises in America.

Dr. Caspar Wistar [b. 1761, d. 1818] was also a leader, and was at various times professor of chemistry and anatomy. His contributions to natural history were descriptions of bones of *Megalonyx* and other mammals, a study of the human ethmoid, and experiments on evaporation. He was long Vice-President of the Philosophical Society, and in 1815 succeeded Jefferson in its presidency. The Wistar Anatomical Museum of the University and the beautiful climbing shrub *Wistaria* are among the memorials to his name.*

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* BARTON'S Memoirs of Rittenhouse, p. 614.

† THACHER. American Medical Biography, i. p. 408.

‡ This eventually became the property of the University. See Barton's Rittenhouse, p. 377. Trans. Amer. Phil. Soc., ii, p. 368.

Still another memorial of the venerable naturalist may perhaps be worthy of mention as an illustration of the social conditions of science in Philadelphia in early days. A traveller visiting the city in 1829 thus described this institution, which was continued until the late war, and then discontinued, but has been resumed within the last year:

> "Dr. Wistar in his lifetime had a party of his literary and scientific friends at his house, one evening in each week, and to this party strangers visiting the city were also invited. When he died, the same party was continued, and the members of the Wistar party, in their turn, each have a meeting of the club at his house, on some Saturday night in the year. This club consists of the men most distinguished in science, art, literature, and wealth in the city. It opens at early candle-light, when not only the members themselves appear, but they bring with them all the strangers of distinction in the city."†

The "Wistar parties" were continued up to the beginning of the civil war in 1861, and have been resumed since 1887. A history of these gatherings would cover a period of three-quarters of a century at the least, and could be made a most valuable and entertaining contribution to scientific literature.

Packard, in his History of Zoölogy,‡ states that zoölogy, the world over, has sprung from the study of human anatomy, and that American zoölogy took its rise, and was fostered chiefly, in Philadelphia, by the professors in the medical schools.

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* Hosack: Tribute to the Memory of Wistar, New York, 1818.

† Atwater: Remarks made on a tour to Prairie du Chiēn; thence to Washington City, in 1829. Columbus, 1831, p. 238.

‡ Standard Natural History, pp. lxii–lxxii.

It was fully demonstrated, I think, in my former address, that there were good zoölogists in America long before there were medical schools, and that Philadelphia was *not* the cradle of American natural history; although, during its period of political pre-eminence, immediately after the Revolution, scientific activities of all kinds centred in that city. As for the medical schools it is at least probable that they have spoiled more naturalists than they have fostered.

Dr. Adam Kuhn [b. 1741, d. 1817] was the professor of botany in 1768 *—the first in America—and was labeled by his contemporaries "the favorite pupil of Linnæus." Professor Gray, in a recent letter to the writer, refers to this saying as a "myth;" and it surely seems strange that a disciple beloved by the great Swede could have done so little for botany. Barton, in a letter, in 1792, to Thunberg, who then occupied the seat of Linnæus in the University of Upsala, said:

> "The electricity of your immortal Linné has hardly been felt in this Ultima Thule of science. Had a number of the pupils of that great man settled in North America its riches would have been better known. But, alas! the only one pupil of your predecessor that has made choice of America as the place of his residence has added nothing to the stock of natural knowledge."†

The Rev. Nicholas Collin, Rector of the Swedish Churches in Pennsylvania, was a fellow-countryman and acquaintance of Linnæus ‡ and an accomplished botanist, having been one of the editors of Muhlenberg's work upon the grasses and an early writer on American linguistics. He read before the Philosophical Society, in 1789, "An Essay on those inquiries in

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* See p. 99, *ante*.

† B. S. Barton, in Transactions American Philosophical Society, iii, p. 339.

‡ "I often heard the great Linnæus wish that he could have explored the continent of North America." Collin: Trans. Amer. Phil. Soc., iii, p. xv.

Natural Philosophy which at present are most beneficial to the United States of North America," which was the first attempt to lay out a systematic plan for the direction of scientific research in America. One of the most interesting suggestions he made was that the Mammoth was still in existence.

> " The vast Mahmot," said he, " is perhaps yet stalking through the western wilderness; but if he is no more let us carefully gather his remains, and even try to find a new skeleton of this giant, to whom the elephant was but a calf." *

Gen. Jonathan Williams, U. S. A. [b. 1750, d. 1815], was first superintendent of the Military Academy at West Point and " father of the corps of engineers." He was a nephew of Franklin, and his secretary of legation in France, and, after his return to Philadelphia, was for many years a judge of the court of common pleas, his military career not beginning till 1801. This versatile man was a leading member of the Philosophical Society and one of its Vice-Presidents. His paper " On the Use of the Thermometer in Navigation " was one of the first American contributions to scientific seamanship.

The Rev. Dr. John Ewing [b. 1732, d. 1802], also a Vice-President, was Provost of the University. He had been one of the observers of the transit in 1769, of which he published an account in the Transactions of the Philosophical Society. He early printed a volume of lectures on Natural Philosophy, and was the strongest champion of John Godfrey, the Philadelphian, in his claim to the invention of the reflecting quadrant.†

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* *Id.*, p. xxiv.

† "Thomas Godfrey," says a recent authority, "was born in Bristol, Penn., in 1704, and died in Philadelphia in December, 1749. He followed the trade of a glazier in the metropolis, and, having a fondness for mathematical studies, marked such books as he met with, subsequently acquiring Latin, that he might become familiar with the mathematical work in that language. Having obtained a copy of Newton's 'Principia,' he described an improvement he had made in Davis' quadrant to James Logan,

Dr. James Woodhouse [b. 1770, d. 1809] was author and editor of several chemical text-books and Professor of Chemistry in the University, a position which he took after it had been refused by Priestley. He made experiments and observations on the vegetation of plants, and investigated the chemical and medical properties of the persimmon tree. He it was who first demonstrated the superiority of anthracite to bituminous coal by reason of its intensity and regularity of heating power.*

The Rev. Ebenezer Kinnersley [b. in Gloucester, England, Nov. 30, 1711, d. in Philadelphia, July 4, 1778] survived the Revolution, though, in his latter years, not a contributor to science. The associate of Franklin in "the Philadelphia Experiments" in electricity, his discoveries were famous in Europe as well as in America.† It is claimed that he originated the theory of the positive and negative in electricity; that he first demonstrated the passage of electricity through water; and that he first discovered that heat could be produced by electricity; besides inventing numerous mechanical devices of scientific interest. From 1753 to 1772 he was connected with the University of Pennsylvania, where there may still be seen a window dedicated to his memory.

Having already referred to the history of scientific instruction in America,‡ and shown that Hunter lectured on comparative anatomy in Newport in 1754; Kuhn on Botany, in Philadelphia, in 1768, Waterhouse on natural history and botany, at Cambridge, in 1788; and some unidentified scholars upon chemistry and natural history, in Philadelphia, in 1785, it would seem unjust not to speak of Kinnersley's career as a lecturer.

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who was so impressed that he at once addressed a letter to Edmund Halley in England, giving a full description of the construction and uses of Godfrey's instrument."

* SILLIMAN: American Contributions to Chemistry, p. 13.

† See *Priestley's* History of Electricity.

‡ P. 99, *ante*.

He seems to have been the first to deliver public scientific lectures in America, occupying the platform in Philadelphia, Newport, New York, and Boston, from 1751 to the beginning of the Revolution. The following advertisement was printed in the "Pennsylvania Gazette" for April 11, 1751:

> NOTICE is hereby given to the *Curious* that Wednesday next Mr. Kinnersley proposes to begin a Course of Experiments on the newly-discovered *Electrical Fire*, containing not only the most curious of those that have been made and published in Europe, but a considerable Number of New Ones lately made in this City, to be accompanied with methodical *Lectures* on the Nature and Properties of that Wonderful Element.

Francis Hopkinson [b. 1737, d. 1791], signer of the Declaration of Independence, was treasurer of the Philosophical Society, and among other papers communicated by him was one in 1783, calling attention to the peculiar worm parasitic in the eye of a horse. The "horse with a snake in its eye" was on public exhibition in Philadelphia in 1782, and was the object of much attention, for the nature and habits of this peculiar *Filaria* were not so well understood then as now.

The father of Francis, Thomas Hopkinson [b. in London, 1709, d. in Philadelphia, 1751], who was overlooked in my previous address, deserves, at least, a passing mention. Coming to Philadelphia in 1731 he became lawyer, prothonotary, Judge of the Admiralty, and member of the Provincial Council. As an incorporator of the Philadelphia Library Company, and original trustee of the College of Philadelphia, and first President of the American Philosophical Society in 1743, his public spirit is worthy of our admiration. He was associated with Kinnersley and Franklin in the "Philadelphia Experiments;" and Franklin said of him:

"The power of points to throw off the electrical fire was first communicated to me by my ingenious friend, Mr. Thomas Hopkinson."*

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* WILSON & FISKE: Cyclopædia of American Biography, iii, 260.

The name of Philip Syng is also mentioned in connection with the Philadelphia experiments, and it would be well if some memorials of his work could be placed upon record.

William Bartram [b. 1739, d. 1823] was living in the famous botanical garden at Kingsessing, which his father, the old King's botanist, had bequeathed him in 1777. He was for some years professor of botany in the Philadelphia college, and in 1791 printed his charming volume descriptive of his travels in Florida, the Carolinas, and Georgia. The latter years of his life appear to have been devoted to quiet observation. William Bartram has been, perhaps, as much underrated as John Bartram has been unduly exalted. He was one of the best observers America has ever produced, and his book, which rapidly passed through several editions in English and French, is a classic and should stand beside White's "Selborne" in every naturalist's library. Bartram was doubtless discouraged early in his career by the failure of his patrons in London to make any scientific use of the immense botanical collections made by him in the South before the Revolution, which, many years later, was lying unutilized in the Banksian herbarium. Coues has called attention very emphatically to the merits of his bird work, which he pronounces "the starting-point of a distinctly American school of ornithology." Two of the most eminent of our early zoölogists, Wilson and Say, were his pupils; the latter his kinsman, and the former his neighbor, were constantly with him at Kingsessing and drew much of their inspiration from his conversation. "Many birds which Wilson first fully described and figured were really named and figured by Bartram in his Travels, and several of his designations were simply adopted by Wilson."*

Bartram's "Observations on the Creek and Cherokee Indians"† was an admirable contribution to ethnography, and his general observations were of the highest value.

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* COUES: Key to North American Birds, p. xvi

† Trans. Am. Ethnological Society, iii, 1851.

In the introduction to his "Travels," and interspersed through this volume, are reflections which show him to have been the possessor of a very philosophic and original mind.

His "Anecdotes of an American Crow" and his "Memoirs of John Bartram"* were worthy products of his pen, while his illustrations to Barton's "Elements of Botany" show how facile and truthful was his pencil.

His love for botany was such, we are told, that he wrote a description of a plant only a few minutes before his death, a statement which will be readily believed by all who know the nature of his enthusiasm. Thus, for instance, he wrote of the Venus's Fly Trap:

> "Admirable are the properties of the extraordinary Dionæa muscipula! See the incarnate lobes expanding; how gay and sportive they appear! ready on the spring to entrap incautious, deluded insects! What artifice! There! behold one of the leaves just closed upon a struggling fly; another has gotten a worm; its hold is sure; its prey can never escape—carnivorous vegetable! Can we, after viewing this object, hesitate for a moment to confess that vegetable beings are endowed with some sensible faculties or attributes similar to those that dignify animal nature? They are living, organical, and self-moving bodies; for we see here in this plant motion and volition."†

Moses Bartram, a cousin of William, and also a botanist, was also living near Philadelphia, and in 1879 published "Observations on the Native Silk Worms of North America," and Humphrey Marshall [1722–1801], the farmer-botanist, had a botanical garden of his own, and in 1785 published "The American Grove—Arbustrium Americanum"—a treatise on the forest trees and shrubs of the United States, which was the first strictly

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* Nicholson's Journal, 1805.

† Travels, 1793, p. xiv.

American botanical book, and which was republished in France a few years later in 1789.

Gotthilf Muhlenberg [b. 1753, d. 1815], a Lutheran clergyman, living at Lancaster, was an eminent botanist, educated in Germany, though a native of Pennsylvania. His "Flora of Lancaster" was a pioneer work In 1813 he published a full catalogue of the Plants of North America, in which about 2,800 species were mentioned. He supplied Hedwig with many of the rare American mosses, which were published either in "Stirpes Cryptogamicæ" of that author or in the "Species Muscorum." To Sir J. E. Smith and Mr. Dawson Turner he likewise sent many plants. He made extensive preparations, writing a general flora of North America, but death interfered with his project. The American Philosophical Society preserves his herbarium, and the moss *Funeria Muhlenbergii*, the violet, *Viola Muhlenbergii*, and the grass *Muhlenbergia*, are among the memorials to his name.*

To Pennsylvania, but not to Philadelphia, came, in 1794, Joseph Priestley (1733–1804), the philosopher, theologian, and chemist. Although his name is more famous in the history of chemistry than that of any living contemporary, American or European, his work was nearly finished before he left England. He never entered into the scientific life of the country which he sought as an exile, and of which he never became a citizen, and he is not properly to be considered an element in the history of American science.

His coming, however, was an event of considerable political importance; and William Cobbett's "Observations on the Emigration of Doctor Joseph Priestley. By Peter Porcupine," was followed by several other pamphlets equally vigorous in expression. McMaster is evidently unjust to some of the public men who welcomed Priestley to America, though no one will deny that there were unprincipled demagogues in America in the year of grace 1794. Jefferson was undoubtedly sincere when he wrote to him the words quoted elsewhere in this address.

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* Hooker: On the Botany of America. Edinburgh Journal of Science, iii, p. 103, *et seq.*

Another eminent exile, welcomed by Jefferson, and the writer, at the President's request, of a work on national education in the United States, was M. Pierre Samuel Dupont de Nemours [b. in Paris, 1799, d. 1817]. He was a member of the Institute of France, a statesman, diplomatist, and political economist, and author of many important works. He lived in the United States at various times, from 1799 to 1817, when he died near Wilmington, Delaware. Like Priestley, he was a member of the American Philosophical Society, and affiliated with its leading members.

The gunpowder works near Wilmington, Delaware, founded by his son in 1798, are still of great importance, and the statue of one of his grandsons, an Admiral in the U. S. Navy, adorns one of the principal squares in the National Capital.

Among other notable names on the roll of the society, in the last century, were those of Gen. Anthony Wayne and Thomas Payne. His Excellency General Washington was also an active member, and seems to have taken sufficient interest in the society to nominate for foreign membership the Earl of Buchan, President of the Society of Scottish Antiquarians, and Dr. James Anderson, of Scotland.

The following note written by Washington is published in the Memoirs of Rittenhouse :

> "The President presents his compliments to Mr. Rittenhouse, and thanks him for the attention he has given to the case of Mr. Anderson and the Earl of Buchan.
>
> "SUNDAY AFTERNOON, 20*th April*, 1794."

Of all the Philadelphia naturalists of those early days, the one who had the most salutary influence upon the progress of science was, perhaps, Benjamin Smith Barton [b. 1766, d. 1815.] Barton was the nephew of Rittenhouse, and the son of the Rev. Thomas Barton, a learned Episcopal Clergyman of Lancaster, who was one of the earliest members of the Philosophical Society, and a man accomplished in science.

He studied at Edinburgh and Göttingen, and at the age of 19, in 1785, he was the assistant of Rittenhouse and Ellicott, in the work of establishing the western boundary of Pennsylvania, and soon after was sent to Europe, whence, having pursued an extended course of scientific and medical study, he returned in 1789, and was elected professor of natural history and botany in the University of Pennsylvania. He was a leader in the Philosophical Society, and the founder of the Linnæan Society of Philadelphia, before which, in 1807, he delivered his famous "Discourse on some of the Principal Desiderata in Natural History," which did much to excite an intelligent popular interest in the subject. His essays upon natural history topics were the first of the kind to appear in this country. He belonged to the school of Gilbert White and Benjamin Stillingfleet, and was the first in America of a most useful and interesting group of writers, among whom may be mentioned John D. Godman, Samuel Lockwood, C. C. Abbott, Nicholas Pike, John Burroughs, Wilson Flagg, Ernest Ingersoll, the Rev. Dr. McCook, Hamilton Gibson, Maurice Thompson, and W. T. Hornaday, as well as Matthew Jones, Campbell Hardy, Charles Waterton, P. H. Gosse, and Grant Allen, to whom America and England both have claims.

Barton published certain descriptive papers, as well as manuals of botany and materia medica, but in latter life had become so absorbed in medical affairs that he appears to have taken no interest in the struggles of the infant Academy of Natural Sciences, which was founded three years before his death, but of which he never became a member.

His nephew and successor in the Presidency of the Linnæan Society and the University Professorship, William P. C. Barton [b. 1786, d. 1856], was a man of similar tendencies, who in early life published papers on the flora of Philadelphia [Floræ Philadelphiæ Prodromus, 1815], but later devoted himself chiefly to professional affairs, writing copiously upon materia medica and medical botany.

The admirers of Benjamin Smith Barton have called him "the father of American Natural History," but I cannot see the propriety of this designation, which is equally applicable to Mitchill or Jefferson, and perhaps still more so to Peter Collinson, of London. The praises of Barton have been so well and so often sung that I do not feel guilty of injustice in passing him briefly by.*

The most remarkable naturalist of those days was Rafinesque, [b. 1784, d. 1872], a Sicilian by birth, who came to Philadelphia in 1802.

Nearly fifty years ago this man died, friendless and impoverished, in Philadelphia. His last words were these: "Time renders justice to all at last." Perhaps the day has not yet come when full justice can be done to the memory of Constantine Rafinesque, but his name seems yearly to grow more prominent in the history of American zoölogy. He was in many respects the most gifted man who ever stood in our ranks. When in his prime he far surpassed his American contemporaries in versatility and comprehensiveness of grasp. He lived a century too soon. His spirit was that of the present period. In the latter years of his life, soured by disappointments, he seemed to become unsettled in mind, but as I read the story of his life his eccentricities seem to me the outcome of a boundless enthusiasm for the study of nature. The picturesque events of his life have been so well described by Jordan,* Chase,† and Audubon‡ that they need not be referred to here. The most satisfactory gauge of his abilities is perhaps his masterly "Survey of the Progress and Actual State of Natural Sciences in the United States of America," printed in 1817.§ His own sorrowful estimate of the outcome of his mournful career is very touching:

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* W. P. C. Barton: Biography of Benjamin S. Barton, Philadelphia, 1815

> "I have often been discouraged, but have never despaired long. I have lived to serve mankind, but have often met with ungrateful returns. I have tried to enlarge the limits of knowledge, but have often met with jealous rivals instead of friends. With a greater fortune I might have imitated Humboldt or Linnæus."

Dr. Robert Hare [b. 1781, d. 1858] began his long career of usefulness in 1801, at the age of twenty, by the invention of the oxy-hydrogen blow-pipe. This was exhibited at a meeting of the Chemical Society of Philadelphia in 1801.‖

This apparatus was perhaps the most remarkable of his original contributions to science, which he continued without interruption for more than fifty years. It belongs to the end of the post-revolutionary period, and is therefore noticed, although it is not the purpose of this essay to consider in detail the work of the specialists of the present century.

Dr. Hugh Williamson [b. Dec. 5, 1735, d., in New York, May 22, 1719] was a prominent but not particularly useful promoter of science, a writer rather than a thinker. His work has already been referred to. The names of Maclure, who came to Philadelphia about 1797, the Rev. John Heckewelder, and Albert Gallatin [b. 1761, d. in 1849], a native of Switzerland, a statesman and financier, subsequently identified with the scientific circles of New York, complete the list of the Philadelphia savans of the last century.

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* JORDAN: Bulletin xv, U. S. National Museum: Science Sketches, p. 143.

† CHASE: Potter's American Monthly, vi, pp. 97–101.

‡ AUDUBON: The Eccentric Naturalist < Ornithological Biography, p. 455.

§ Amer. Monthly Magazine, ii, 81.

‖ Amer. Month. Mag., i, 80.

There is not in all American literature a passage which illustrates the peculiar tendencies in the thought of this period so thoroughly as Jefferson's defense of the country against the charges of Buffon and Raynal, which he published in 1783, which is particularly entertaining because of its almost pettish depreciation of our motherland.

> "On doit etre etonné" (says Raynal) "que l'Amerique n'ait pas encore produit un bon poëte, un habile mathematicien, un homme de génie dans un seul art ou un seule science."
>
> "When we shall have existed a people as long as the Greeks did before they produced a Homer, the Romans a Virgil, the French a Racine and Voltaire, the English a Shakespeare and Milton, should this reproach still be true, we will inquire from what unfriendly causes it has proceeded that the other countries of Europe and quarters of the earth shall not have inscribed any name on the rôle of poets.
>
> "In war we have produced a *Washington* whose name will in future ages assume its just station among the celebrated worthies of the world, when that wretched philosophy shall be forgotten which would have arranged him among the degeneracies of nature.
>
> "In physics we have produced a *Franklin*, than whom no one of the present age has made more important discoveries, nor has enriched philosophy with more, or more ingenious, solutions of the phænomena of nature.
>
> "We have supposed *Mr. Rittenhouse* second to no astronomer living; that in genius he must be the first because he is self-taught. He has not indeed made a world; but he has by imitation approached nearer its Maker than any man who has lived from the creation to this day. There are various ways of keeping the truth out of sight. Mr. Rittenhouse's model of the planetary system has the plagiary appellation of an orrery; and the quadrant invented by *Godfrey*, an American also, and with the aid of which the European nations traverse the globe, is called Hadley's quadrant.
>
> "We calculate thus: The United States contain three millions of inhabitants; France twenty millions; and the British Islands ten. We produce a Washington, a Franklin, a Rittenhouse. France then should have half a dozen in each of these lines, and Great Britain half that number, equally eminent. It may be true

> that France has; we are but just becoming acquainted with her, and our acquaintance so far gives us high ideas of the genius of her inhabitants.
>
> "The present war having so long cut off all communications with Great Britain, we are not able to make a fair estimate of the state of science in that country. The spirit in which she wages war is the only sample before our eyes, and that does not seem the legitimate offspring either of science or civilization. The sun of her glory is fast descending to the horizon. Her philosophy has crossed the channel, her freedom the Atlantic, and herself seems bearing to that awful dissolution whose issue is not given human forethought to scan."*

This was one phase of public sentiment. Another, no less instructive, is that shown forth in the publications of Jefferson's fierce political opponents in 1790, paraphrased, as follows, by McMaster in his "History of the People of the United States:"

> "Why, it was asked, should a philosopher be made President? Is not the active, anxious, and responsible station of Executive illy suited to the calm, retired, and exploring tastes of a natural philosopher? Ability to impale butterflies and contrive turn-about chairs may entitle one to a college professorship, but it no more constitutes a claim to the Presidency than the genius of Cox, the great bridge-builder, or the feats of Ricketts, the equestrian. Do not the pages of history teem with evidence of the ignorance and mismanagement of philosophical politicians? John Locke was a philosopher, and framed a constitution for the colony of Georgia, but so full was it of whimsies that it had to be thrown aside. Condorcet, in 1793, made a constitution for France, but it contained more absurdities than were ever before piled up in a system of government, and was not even tried. Rittenhouse was another philosopher; but the only proof he gave of political talents was suffering himself to be wheedled into the presidency of the Democratic Society of Philadelphia. But suppose that the title of philosopher is a good claim to the Presidency, what claim has Thomas Jefferson to the title of philosopher? Why, forsooth!
>
> "He has refuted Moses, dishonored the story of the Deluge, made a penal code, drawn up a report in weights and measures, and speculated profoundly on the primary causes of the difference between the whites and blacks. Think of such a man as President! Think of a foreign minister surprising him in the act of anatomizing the kidneys and glands of an African to find out why the negro is black and odoriferous!

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* Notes on Virginia, 1788, pp. 69–71.

"He has denied that shells found on the mountain tops are parts of the great flood. He has declared that if the contents of the whole atmosphere were water, the land would only be overflowed to the depth of fifty-two and a half feet. He does not believe the Indians emigrated from Asia.

"Every mail from the South brought accounts of rumblings and quakes in the Alleghanies, and strange lights and blazing meteors in the sky. These disturbances in the natural world might have no connection with the troubles in the political world; nevertheless it was impossible not to compare them with the prodigies all writers of the day declare preceded the fatal Ides of March."

X.

In New York, although a flourishing medical school had been in existence from 1769, there was an astonishing dearth of naturalists until about 1790. Governor Colden, the botanist and ethnologist, had died in 1776, and the principal medical men of the city, the Bards, Clossy, Jones, Middleton, Dyckman, and others, confined their attention entirely to professional studies. A Philosophical Society was born in 1787, but died before it could speak. A Society for the Promotion of Agriculture, Arts, and Manufactures, organized in 1791, was more successful, but not in the least scientific. Up to the end of the century New York State had but six men chosen to membership in the American Philosophical Society, and, up to 1809, but five in the American Academy. Leaders, however, soon arose in Mitchill, Clinton, and Hosack.

Samuel Latham Mitchill, the son of a Quaker farmer [b. 1764, d. 1831], was educated in the medical schools of New York and Edinburgh, and in 1792 was appointed Professor of Chemistry, Natural History, and Philosophy in Columbia College. Although during most of his long life a medical professor and editor, and for many years representative and senator in Congress, he continued active in the interests of general science. He made many contributions to systematic natural history, notably a History of the Fishes of New York, and his edition of Bewick's

"General History of Quadrupeds," published in New York in 1804, with notes and additions, and some figures of American animals, was the earliest American work of the kind. He was the first in America to lecture upon geology, and published several papers upon this science. His "Mineralogical Exploration of the banks of the Hudson River" in 1796, under the "Society for the Promotion of Agriculture, Manufactures, and Useful Arts," founded by himself, was our earliest attempt at this kind of research, and in 1794 he published an essay on the "Nomenclature of the New Chemistry," the first American paper on chemical philosophy, and engaged in a controversy with Priestley, in defence of the nomenclature of Lavoisier, which he was the first American to adopt.

His discourse on "The Botanical History of North and South America" was also a pioneer effort. He was an early leader in ethnological inquiries and a vigorous writer on political topics. His "Life of Tammany, the Indian Chief" (New York, 1795), is a classic, and he was well known to our grandfathers as the author of "An Address to the Fredes or People of the United States," in which he proposed that "Fredonia" should be adopted as the name of the nation.

Dr. Mitchill was a poet,* and a humorist, and a member of the literary circles of his day. In "The Croakers" Rodman Drake thus addressed him as "The Surgeon General of New York:"

"It matters not how high or low it is
Thou knowest each hill and vale of knowledge,
Fellow of forty-nine societies
And lecturer in Hosack's College."

Fitz-Greene Halleck also paid his compliments in the following terms:

"Time was when Dr. Mitchill's word was law,
When Monkeys, Monsters, Whales and Esquimaux,
Asked but a letter from his ready hand,
To be the theme and wonder of the land."

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* Examples of his verses may be found in Duyckinck's Cyclopædia of American Literature.

These and other pleasantries, of which many are quoted in Fairchild's admirable "History of the New York Academy of Sciences," gives us an idea of the provinciality of New York sixty years ago, when every citizen would seem to have known the principal local representatives of science, and to have felt a sense of personal proprietorship in him and in his projects.

Mitchill was a leader in the New York Historical Society; founder of the Literary and Philosophical Society, and of its successor, the Lyceum of Natural History, of which he was long president. He was also President of the New York Branch of the Linnæan Society of Paris, and of the N. Y. State Medical Society, and Surgeon-General of the State Militia; a man of the widest influence and universally beloved. He served four terms in the House of Representatives, and was five years a member of the U. S. Senate.*

DeWitt Clinton [b. 1769, d. 1828], statesman and philanthropist, U. S. Senator, and Governor of New York, was a man of similar tastes and capacities. What Benjamin Franklin was to Philadelphia in the middle of the eighteenth century DeWitt Clinton was to New York in the beginning of the nineteenth. He was the author of the Hibernicus "Letters on the Natural History and Internal Resources of the State of New York" (New York, 1822), a work of originality and merit. As President of the Literary and Philosophical Society he delivered in 1814 an "Introductory Discourse," which, like Barton's in

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* See FRANCIS, JOHN W. Life of Dr. Mitchill, in Williams's American Medical Biography, pp. 401–411, and eulogy in Discourse in Commemoration of 53d Anniversary of N. Y. Hist. Soc., 1857, 56–60; and in his Old New York; also—

Sketch by H. L. Fairchild in History of the New York Academy of Sciences, 1887, pp. 57–67; also Dr. Mitchill's own pamphlet: Some of the Memorable Events and Occurrences in the Life of Samuel S. Mitchill, of New York, from the year 1786 to 1827.

A biography by Akerly was in existence, but has never been printed.

Numerous portraits are in existence, which are described by Fairchild.

Philadelphia, ten years before, was productive of great good. It was, moreover, laden with the results of original and important observations in all departments of natural history. Another important paper was his "Memoirs on the Antiquities of Western New York" printed in 1818.

Clinton's attention was devoted chiefly to public affairs, and especially to the organization of the admirable school system of New York and other internal improvements. He did enough in science, however, to place him in the highest ranks of our early naturalists.*

Hosack has been referred to elsewhere as a pioneer in mineralogy and the founder of the first botanic garden. He was long president of the Historical Society, and exercised a commanding influence in every direction. His researches were, however, chiefly medical.

Samuel Akerly [b. 1785, d. 1845], the brother-in-law of Mitchill, a graduate of Columbia College, 1807, was an industrious worker in zoölogy and botany and the author of the "Geology of the Hudson River." John Griscom [b. 1774, d. 1852], one of the earliest teachers of chemistry, began in 1806 a career of great usefulness. "For thirty years," wrote Francis, "he was the acknowledged head of all other teachers of chemistry among us (in New York), and he kept pace with the flood of light which Davy, Murray, Gaylussac, and Thenard, and others shed on the progress of chemical philosophy at that day." About 1820 he went abroad to study scientific institutions, and his charming book, 'A Year in Europe,' supplemented by his regular contributions to *Silliman's Journal*, commenting on scientific affairs in other countries, did much to stimulate the growth of scientific and educational institutions in America.

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* HOSACK: Memoirs of DeWitt Clinton. New York, 1829. RENWICK: Life of DeWitt Clinton. New York, 1840. CAMPBELL: Life and Writings of DeWitt Clinton. New York, 1849.

Francis tells us that he was for thirty years the acknowledged head of the teachers of chemistry in New York.*

A zealous promoter of zoölogy in those days was F. Adrian Vanderkemp, of Oldenbarnavelt, New York, who in 1795, we are told, delivered an address before an Agricultural Society in Whitesburg, N. Y., in which he offered premiums for essays upon certain subjects, among which was one "for the best anatomical and historical account of the moose, fifty dollars, or for bringing one in alive, sixty dollars."†

Having mentioned several American naturalists of foreign birth, it may not be out of place to refer to the American origin of an English zoölogist of high repute, Dr. Thomas Horsfield, born in Philadelphia in 1773, and after many years in the East became, in 1820, a resident of London, where he died in 1859. His name is prominent among those of the entomologists, botanists, and ornithologists of this century, especially in connection with Java.

XI.

In New England, science was more highly appreciated than in New York. Massachusetts had in John Adams a man who, like Franklin and Jefferson, realized that scientific institutions were the best protection for a democratic government, and to his efforts America owes its second scientific society—the American Academy of Arts and Sciences, founded in 1780. When Mr. Adams travelled from Boston to Philadelphia, in the days just before the Revolution, he several times visited at Norwalk, we are told, a curious collection of American birds and insects made by Mr. Arnold. "This was afterwards sold to Sir Ashton Lever, in whose apartments in London Mr. Adams saw it again, and felt a new regret at our imperfect knowledge of the productions of the three kingdoms of nature in our land. In France his visits to the museums and other establishments, with the inquiries of Academicians and other men of science and letters respecting this country, and their encomiums on the Philosophical Society of Philadelphia, suggested to him the idea of engaging his native State to do something in the same good but neglected cause."*

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* Griscom, John H.: Memoir of John Griscom. New York, 1859.

† DeWitt Clinton, in Trans. Lt. Phil. Soc. N. Y., p. 59.

The Academy, from the first, was devoted chiefly to the physical sciences, and the papers in its memoirs for the most part relate to astronomy and meteorology.

Among its early members I find the names of but two naturalists: The Rev. Manasseh Cutler, pastor of Ipswich Hamlet, one of the earliest botanists of New England,† and William Dandridge Peck [b. 1763, d. 1882], the author of the first paper on systematic zoölogy ever published in America, a "Description of four remarkable fishes, taken near the Piscataqua in New Hampshire," published in 1794.‡ Peck, after graduating at Harvard, lived at Kittery, N. H., and first became interested in natural history by reading a wave-worn copy of Linné's "System of Nature," which he obtained from the ship which was wrecked near his house. He became a good entomologist, and communicated much valuable material to Kirby in England, and was also one of our first writers on the fungi. He was the first to occupy the chair of natural history in Harvard University, to which he was appointed in 1800.

The Rev. Dr. Jedediah Morse [b. 1761, grad. Yale, 1783, d. 1826] was the earliest of American geographers, and appears, especially in the later gazetteers published by him, to have printed important facts concerning the number and geographical distribution of the various Indian tribes.

The Connecticut Academy of Arts and Sciences was founded in 1799, one of the chief promoters being President Dwight [b. 1752, d. 1817], whose "Travels in New England and New York," printed in 1821, abounds with scientific observations.

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* KIRTLAND: Mem. Amer. Acad. New Series, vol. 1, p. xxii.

† See previous address, p. 95.

‡ Mem. Amer. Acad. Sci., ii, Part ii, p. 46. 1797.

Another was E. C. Herrick [b. 1811, d. 1862], for many years librarian and subsequently treasurer of Yale College, whose observations upon the aurora, made in the latter years of the last century, are still frequently quoted; and later an active investigator of volcanic phenomena, and the author of a treatise on the Hessian fly and its parasites, the results of nine years' study; and of another on the existence of a planet between Mercury and the sun.

Benjamin Silliman [b. in Trumbull, Conn., Aug. 8, 1779, d. in New Haven, Nov. 27, 1869], who, in 1802, became Professor of Chemistry at Yale, began there his career of usefulness as an organizer, teacher, and critic. One of his introductions to popular favor was the paper which he, in conjunction with Prof. Kingsley, published, "An account of the meteor which burst over Weston, in Connecticut, in December, 1807." This paper attracted attention everywhere, for the nature of meteors was not well understood in those days. Jefferson was reputed to have said in reference to it, "that it was easier to believe that two Yankee professors could lie than to admit that stones could fall from heaven;" but I think this must be pigeon-holed with the millions of other slanders to which Jefferson was subjected in those days. I find in the papers by Rittenhouse and Madison, published twenty years before, by the Philosophical Society, matter-of-fact allusions to the falling of meteors to the earth.

Silliman was the earliest of American scientific lecturers who appeared before popular audiences, and, as founder and editor of the Journal of Science, did a service to science, the value of which is beyond estimate or computation.

Benjamin Waterhouse, Professor of the Theory and Practice of Medicine in Harvard, 1783–1812, was one of the earliest teachers of natural botany in America, and the author of a poem entitled "The Botanist." * The Rev. Jeremy Belknap [b. 1744, d. 1798], in his "History of New Hampshire," and the Rev. Samuel Williams [b. 1743, d. 1817], in his "Natural and Civil History of Vermont,"† made contributions to local natural history, and Capt. Jonathan Carver [b. 1732, d. 1780], in his "Travels through the Interior Parts of America," ‡ gave some meagre information as to the zoölogy and botany of regions previously unknown.

In the South the prestige of colonial days seemed to have departed. Except Jefferson, the only naturalist in Virginia was Dr. James Greenway, of Dinwiddie Co., a botanist of some merit. Mitchell returned to England before the Revolution, and Garden followed in 1784. H. B. Latrobe, of Baltimore, was an amateur ichthyologist, and Dr. James MacBride, of Pineville, S. C. [b. 1784, d. 1817], was an active botanist. Dr. Lionel Chalmers [b. 1715, d. 1777], who was for many years the leader of scientific activity in South Carolina, was omitted in the previous address. A graduate of Edinburgh, he was for forty years a physician in Charleston. He recorded observations on meteorology from 1750 to 1760, the foundation of his "Treatise on the Weather and Diseases of South Carolina" [London, 1776], and published also valuable papers on pathology. He was the host and patron of many naturalists, such as the Bartrams.

There was no lack of men in the South who were capable of appreciating scientific work. Virginia had fourteen members in the American Philosophical Society from 1780 to 1800, while Massachusetts and New York had only six each, the Carolinas had eight, and Maryland six. The population of the South was, however, widely dispersed and no concentration of effort

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* Biography in Polyanthus, vol. ii.

† Walpole, N. H., 1794, 8vo, p. 416.

‡ 1778.

was possible. To this was due, no doubt, the speedy dissolution of the Academy of Arts and Sciences founded in Richmond in 1788.*

A name which should, perhaps, be mentioned in connection with this is that of Dr. William Charles Wells, whom it has been the fashion of late to claim as an American. It would be gratifying to be able to vindicate this claim, for Wells was a man of whom any nation might be proud. He was the originator of the generally-accepted theory of the origin of dew, and was also, as Darwin has shown, the first to recognize and announce the theory of evolution by natural selection.† Unfortunately Wells's science was not American science. We might with equal propriety claim as American the art of James Whistler, the politics of Parnell, the fiction of Alexandre Dumas, the essays of Grant Allen, or the science of Rumford and Le Vaillant.

Wells was the son of an English painter, who emigrated, in 1753, to South Carolina, where he remained until the time of the Revolution, when, with other loyalists, he returned to England. He was born during his father's residence in Charleston, but left the country in his minority; was educated at Edinburgh, and though he, as a young physician, spent four years in the United States, he was permanently established in London practice fully twenty-eight years before he read his famous letter before the Royal Society.

The first American naturalist who held definite views as to evolution was, undoubtedly, Rafinesque. In a letter to Dr. Torrey, Dec. 1, 1832, he wrote:

> "The truth is that species, and perhaps genera also, are forming in organized beings by gradual deviations of shapes, forms, and organs taking place in the lapse of time. There is a tendency

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* See previous discourse, p. 98.

† DARWIN: Origin of species, 6th Amer. Ed., p xv. MORSE: Proc. Amer. Assoc. Adv. Science, xxv, p. 141.

to deviation and mutation in plants and animals by gradual steps, at remote, irregular periods. This is a part of the great universal law of *perpetual mutability* in everything."

It is pleasant to remember that both Darwin and Wallace owed much of their insight into the processes of nature to their American explorations. It is also interesting to recall the closing lines, almost prophetic as they seem to-day, of the "Epistle to the Author of the Botanic Garden,"* written in 1798 by Elihu Hubbard Smith, of New York, and prefixed to the American editions of "The Botanic Garden:"

" Where Mississippi's turbid waters glide
And white Missouri pours its rapid tide;
Where vast Superior spreads its inland sea
And the pale tribes near icy empires sway;
Where now Alaska lifts its forests rude
And Nootka rolls her solitary flood.
Hence keen incitement prompt the prying mind
By treacherous fears, nor palsied nor confined;
Its curious search embrace the sea and shore
And mine and ocean, earth and air explore.

" Thus shall the years proceed,—till growing time
Unfold the treasures of each different clime;
Till one vast brotherhood mankind unite
In equal bonds of knowledge and of right;
Thus the proud column, to the smiling skies
In simple majesty sublime shall rise,
O'er ignorance foiled, their triumph loud proclaim,
And bear inscribed, immortal, DARWIN's name."

XII.

During the three decades which made up the post-revolutionary period there were several "beginnings" which may not well be referred to in connection with individuals or localities.

The first book upon American insects was published in 1797, a sumptuously-illustrated work, in two volumes, with 104 colored plates, entitled "The Natural History of the rarer Lepidopterous Insects of Georgia." This was compiled by Sir James E. Smith from the notes and drawings of John Abbot

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* **Erasmus, grandfather of Charles Darwin.**

[b. about 1760], living in England in 1840, an accomplished collector and artist, who had been for several years a resident of Georgia, gathering insects for sale in Europe. Mr. Scudder characterizes him as "the most prominent student of the life histories of insects we have ever had."[*]

There had, however, been creditable work previously done in what our entomologists are pleased to call the biological side of the science. As early as 1768, Col. Landon Carter, of "Sabine Hall," Virginia, prepared an elaborate paper "On the Habits of the Fly-Weevil that destroys the Wheat," which was printed by the American Philosophical Society,[†] accompanied by an extended report by "The Committee of Husbandry." In the same year Moses Bartram presented his "Observations on the native Silk-Worms of North America."[‡]

Organized effort in economic entomology appears to date from the year 1792, when the American Philosophical Society appointed a committee to collect materials for a natural history of the Hessian Fly, at that time making frightful ravages in the wheat-fields, and so much dreaded in Great Britain that the import of wheat from the United States was forbidden by law. The Philosophical Society's committee was composed of Thomas Jefferson, at that time Secretary of State in President Washington's cabinet, Benjamin Smith Barton, James Hutchinson, and Caspar Wistar. In their report, which was accompanied by large drawings, the history of the little marauder was given in considerable detail.

The publication of Wilson's American Ornithology, beginning in 1808, was an event of great importance. It was in 1804 that the author, a schoolmaster near Philadelphia, decided upon his plan. In a letter to Lawson he wrote:

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[*] There is a whole series of quarto or folio volumes in the British Museum done by him, and a few volumes are extant in this country. Besides, all the biological material in Smith-Abbot's Insects of Georgia is his."—*Letter of S. H. Scudder.*

[†] Transactions of the American Philosophical Soc., 1, 274.

[‡] *Ibid.*, p. 294.

> "I am most earnestly bent on pursuing my plan of making a Collection of all the Birds of North America. Now, I don't want you to throw cold water on this notice, Quixotic as it may appear. I have been so long accustomed to the building of Airy Castles and brain Windmills that it has become one of my comforts of life, a sort of rough Bone, that amuses me when sated with the dull drudgery of Life."

I need not eulogize Wilson. Every one knows how well he succeeded. He has had learned commentators and eloquent biographers. Our children pore over the narrative of the adventurous life of the weaver naturalist, and we all are sensible of the charms which his graceful pen has given to the life-histories of the birds.

His poetical productions are immortal, and his lines to the Blue Bird and the Fisherman's Hymn are worthy to stand by the side of Bryant's Waterfowl, Trowbridge's Wood Pewee, Emerson's Titmouse, Thaxter's Sandpiper, and, possibly best of all, Walt. Whitman's Mocking-Bird in "Out of the Cradle endlessly Rocking."

Ichthyology in America dates also from these last years of the century. Garden was our only resident ichthyologist until Peck and Mitchill began their work, but Schœpf, the Hessian military surgeon, printed a paper on the Fishes of New York in 1787, and William Bryant, of New Jersey, and Henry Collins Flagg, of South Carolina, made observations upon the electric eel, in addition to those which Williamson, of North Carolina, laid before the Royal Society in 1775.

Paleontology had its beginning at about the same time in the publication of Jefferson's paper on the Megalonyx or "Great Claw" in 1797.*

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* The first vertebrate fossils were found in Virginia. Samuel Maverick, of Massachusetts, reported to the colony at Boston in 1836 that, at a place

This early study of a fossil vertebrate was followed 20 years later by the first paper which touched upon invertebrates—that by Say on "Fossil Zoölogy," in the first volume of Silliman's Journal. Lesueur seems to have brought from France some knowledge of the names of fossils, and identified many species for the early American geologists.

Stratigraphical and physical geology also came in at this time, and will be referred to later.

The science of mineralogy was brought to America in its infancy. The first course of lectures upon this subject ever given in London was in the winter of 1793–4, by Schmeisser, a pupil of Werner. Dr. David Hosack, then a student of medicine at Edinburgh, was one of his hearers, and inspired by his enthusiasm began at once to form the collection of minerals which he brought to America on his return in 1794, which was the first mineralogical cabinet ever seen on this side of the Atlantic. This collection was exhibited for many years in New York (and in 1821 was given to Princeton College). Howard soon after obtained a select cabinet from Europe, and the museum of the American Philosophical Society acquired the Smith collection. In 1802, Mr. B. D. Perkins, a New York bookseller, brought from London a fine collection, which soon passed into the possession of Yale College, and in 1803 Dr. Archibald Bruce brought over one equally fine, which was made the basis of lectures when in 1806 he became professor of mineralogy in Columbia College. George Gibbs, in 1805, imported the magnificent collection which was long in the custody of the American Geological Society. Seybert, about the same time, brought to Philadelphia the cabinet which in 1813 was bought by the Academy of Natural Sciences and was lectured upon by Troost in 1814.

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on the James River, about sixty miles above its mouth. the colonists had found shells and bones, among these bones that of a whale, eighteen feet below the surface.—Neill's *Virginia Carolorum*, p. 131.

Much of the early botanical exploration was, however, carried out by European botanists: André Michaux [b. near Versailles, 1746, d. Madagascar, 1802], a pupil of the Jussiens and an experienced explorer, was sent by this government, in 1785, to collect useful trees and shrubs for naturalization in France. He remained eleven years; made extensive explorations in the regions then accessible, and as far west as the Mississippi; sent home immense numbers of living plants; and, after his return, in 1796, published his treatise on the American Oaks,* and prepared the materials for his posthumous "Flora Boreali-Americanas."

François André Michaux [b. near Versailles, 1770, d. at Vauréal, 1855] was his father's assistant in these early travels, and in 1802 and 1806 himself made botanical explorations in the Mississippi Valley. His botanical works were of great importance,† especially that known in its English translation as the "North American Sylva," afterward completed by Nuttall, and still the only work of the kind, though soon to be supplemented, we hope, by Professor Sargent's projected monographs.

Frederick Pursh [b. 1774, in Tobolsk, Siberia, d. June 11, 1820, in Montreal, Canada] carried on botanical explorations between 1799 and 1819; living, from 1802 to 1805, in Philadelphia, and from 1807 to 1810 in New York. In 1814 he published in London his "Flora Americæ Septemtrionalis." Pursh's Flora was largely based upon the labors of the American botanists Barton, Hosack, LeConte, Peck, Clayton, Walter, and Lyon, and the botanical collection of Lewis and Clarke, and enumerated about 3,000 species of plants, while Michaux's, printed eleven years before, had only about half that number.

A. von Enslen collected plants at this time, in the South and West, for the Imperial Cabinet in Vienna. C. C. Robin, who travelled from 1802 to 1806 in what are now the Gulf States, wrote a botanical appendix to his Travels, published in 1807, on which Rafinesque founded his "Florula Ludoviciana" (New York, 1817).

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* Histoire des chênes de l'Amerique Septentrionale, 1801; 36 plates.

† Voyage à l'ouest des monte Alléghany, &c. 8vo, pp. 684 Paris, 1808. Histoire des arbres foréstières de l'Amerique, Septentrionale.

Thaddeus Hænke [b. 1761, d. in Cochabamba, Bolivia, 1817] visited Western North America with the Spaniards late in the last century, and made large collections of plants, which were sent to the National Museum of Bohemia, at Prague, and in part described in Presl's "Reliquiæ Hænkianæ," 72 plates.

Archibald Menzies [b. 1754, d. 1842], an English naval surgeon, also collected on our Pacific coast, under Vancouver, in 1780–95, and his plants found their way to Edinburgh and Kew.

Captain Wangenheim, Surgeon Schoepf, of the Hessian contingent of the British army, Olaf Swartz, a Swedish botanical explorer, and others, also gathered plants in these early days, and, in some instances, published in Europe their botanical observations.

Other collectors of this same class were L. A. G. Bosc [1759–1828], who made botanical researches in the Carolinas during the last two years of the century, and returned to France in 1800 with a herbarium of 1,600 species. He also collected fishes, and his name is perpetuated in connection with at least two well-known American fauna. Another was M. Milbert, who collected for Cuvier in New York, Canada, the Great Lake region, and the Mississippi Valley from 1817 to 1823.

The Baron Palisot de Beauvois [b. 1755, d. 1820] came from Santo Domingo to America in 1791. He travelled extensively, and being a zoölogist as well as a botanist, made observations upon our native animals, particularly the reptiles.

It is to him that we owe the most carefully recorded of existing observations of young rattlesnakes crawling down their parent snakes' throats for protection from enemies.

Most of these men did not contribute largely to the advancement of American scientific institutes or affiliate with the naturalists of the day.

Of quite another type was the Count Luigi Castiglioni, who travelled, soon after the Revolution, throughout the Eastern States, and published in 1790 two volumes of his travels.*

The Count Volney [b. at Craon Feb. 3, 1757, d. in Paris April 25, 1820], traveller, statesman, and historian, travelled in this country from 1795 to 1798, and in 1803, while a Senator of the French Republic, published his famous work upon the United States, containing his observations upon its soil and its climate, and upon the Indians, together with the first doctrines of the language of the Miamis,† and also giving a description of the physical and botanical features of the country. Volney was an admirer and intimate friend of Franklin, and it was in his home at Passy, we are told, that he conceived the idea of his most famous book "Les Ruines."‡

Among the traditions of Fauquier county, Virginia, is one which is of interest to naturalists, since it relates to an incident showing the interest of our first President in science:

> "About the year 1796," runs the story, "at the close of a long summer's day, a stranger entered the village of Warrenton. He was alone, and on foot, and his appearance was anything but prepossessing. His garments, coarse and dust-covered, indicated an individual in the humble walks. From a cane across his shoulders was suspended a handkerchief containing his clothing. Stopping in front of Turner's tavern, he took from his hat a paper and handed it to a gentleman standing on the steps; it read as follows:
>
> "The celebrated historian and naturalist
> VOLNEY needs no recommendation from
> "G. WASHINGTON."

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* Viaggio negli Stati Uniti del America Settentrionali.

† Tableau du climat et du sol des Etats-Unis d'Amerique, suivi d'eclaircissements sur la Floride, sur la colonie française a Scioto sur quelques colonies canadiennes, et sur les savages. Paris, 1803. 8vo, 2 vols. 2d edition. Paris. 8vo, 1 vol., pp. 494. Map.

‡ BIGELOW, JOHN: Franklin's Home and Host. in France. *The Century*, May, 1888, p. 743.

In 1801 Jefferson began his eight years of presidency. Since he was the only man of science who has ever occupied the chief magistracy, he has a right to a high place in the esteem of such a society as ours, and I only regret that, having spoken of him at length a year ago, I cannot now discuss his scientific career in all its aspects.

I then spoke of the credit which was due to him for beginning so early as 1780 to agitate the idea of a government exploring expedition to the Pacific, which culminated in the sending out by Congress of the expedition of Lewis and Clarke, in 1803. Captain Lewis [b. 1774, d. 1809], the leader of this expedition, was a young Virginian, the neighbor, and for some years the private secretary, of President Jefferson. He set out in the summer of 1803, accompanied by his associate, Captain Clarke, and twenty-eight men. They entered the Missouri, May 14, 1804, before the middle of the following July had reached the great falls, and by October were upon the western slope, where, embarking in canoes upon the Kouskousky, a branch of the Columbia, they descended to its mouth, where they arrived on the 15th of November, 1805. The following spring they retraced their course, arriving at St. Louis in September.* The results of the expedition were first made known in Jefferson's message to Congress, read February 19, 1806.

The statue of Meriwether Lewis is one of those at the base of the Washington Monument in Richmond, Virginia, and is worthy of the man and his career.

Dr. Asa Gray in a recent letter says:

> "I have reason to think that Michaux suggested to Jefferson the expedition which the latter was active in sending over to the Pacific. I wonder if he put off Michaux for the sake of having it in American hands?"†

The idea of an expedition to the Pacific was one which was likely to occur to any thoughtful American, and was, after all, simply the continuing of a plan as old as the Spanish days of discovery. Jefferson, at all events, was an active promoter of all such enterprises, and after a quarter of a century's effort the expedition was dispatched, while in 1805 Gen. Z. M. Pike was sent to explore the sources of the Mississippi river and the western parts of "Louisiana," penetrating as far west as "Pike's Peak," a name which still remains as a memento of this enterprise.

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* See a complete bibliography of the various reports of this expedition, by Elliott Coues, in the Bulletin of the U. S. Geological Survey.

† See Amer. Journ. Sci., xii, No. 1.

The organization of these early expeditions marked the beginning of one of the most important portions of the scientific work of our government—the investigation of the resources and natural history of the public domain. The expeditions of Lewis and Clarke, and of Pike, were the precursors and prototypes of the magnificent organization now accomplishing so much for science under the charge of Major J. W. Powell.

As early as 1806, Jefferson, inspired by Patterson and Hassler, urged the establishment of a national Coast Survey, and in this was earnestly supported by his Secretary of the Treasury, Albert Gallatin, who drew up a learned and elaborate project for its organization, and an act authorizing its establishment was passed in 1807. During his administration, in 1802, the first scientific school in this country was established, the Military Academy at West Point. The Military Academy was a favorite project of General Washington, who is said to have justified his anxiety for its establishment by the remark that "an army of asses led by a lion is vastly superior to an army of lions led by an ass."

Jefferson has been heartily abused for not gratifying Alexander Wilson's request to be appointed naturalist to Pike's expeditions. It is possible that even in those days administrators were hampered by lack of financial resources. It must also be remembered that in 1804 Wilson was simply an enthusiastic projector of ornithological undertakings, and had done nothing whatever to establish his reputation as an investigator.

One of Jefferson's first official acts was to throw his presidential mantle over Priestley. Two weeks after he became President of the United States he wrote these words:

> "It is with heartfelt satisfaction that, in the first moments of my public action, I can hail you with welcome to our land, tender to you the homage of its respect and esteem, cover you under the protection of those laws which were made for the wise and good like you, and disclaim the legitimacy of that libel on legislators which, under the form of a law, was for some time placed among them."
>
> * * * "Yours is one of the few lives precious to mankind, and for the continuance of which every thinking man is solicitous. Bigots may be an exception. What an effort, my dear sir, of bigotry in politics and religion have we gone through. * * * All advances in science were prescribed as innovations. They pretended to praise and encourage education, but it was to be the education of our ancestors. We were to look backwards, not forwards for improvement; the President (Washington) himself declaring in one of his answers to addresses that we were never to expect to go beyond them in real science. This was the real ground of all the attacks on you; those who live by mystery and *charlatanerie* fearing you would render them useless by simplifying the Christian philosophy, the most sublime and benevolent, but most perverted system that ever shone on man, endeavored to crush your well-earned and well-deserved fame."*

XIII.

With the close of the third decade ended the first third of a century since the Declaration of Independence. We have now passed in review a considerable number of illustrious names and have noted the inception of many worthy undertakings.

"Still, however," in the words of Silliman, "although individuals were enlightened, no serious impression was produced on the public mind; a few lights were, indeed, held out, but they were lights twinkling in an almost impervious gloom."†

This was a state of affairs not peculiar to America. A gloom no less oppressive had long obscured the intellectual atmosphere of the old world. There were a goodly number of men of science, and many important discoveries were being made, but no bonds had yet been formed to connect the interests of the men of science and the men of affairs.

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* Jefferson's Works (T. J. Randolph ed.), 1830, iii, 461.

† Silliman, i, 37.

Speculative science, in the nature of things, can only interest and attract scholarly men. and though its results, concisely and attractively stated, may have a passing interest to a certain portion of every community, it is only by its practical applications that it secures the hearty support of the community at large.

Huxley, in his recent discourse upon "The Advance of Science in the Last Half Century,"* has touched upon this subject in a most suggestive and instructive manner, and has shown that Bacon, with all his wisdom, exerted little direct beneficial influence upon the advancement of natural knowledge, which has after all been chiefly forwarded by men like Galileo and Harvey, Boyle and Newton, "who would have done their work quite as well if neither Bacon nor Descartes had ever propounded their views respecting the manner in which scientific investigation should be pursued."

I think we should look upon Bacon as the prophet of modern scientific thought, rather than its founder. It is no doubt true, as Huxley has said, that his "scientific insight" was not sufficient to enable him to shape the future course of scientific philosophy, but it is scarcely true that he attached any undue value to the practical advantages which the world as a whole, and incidentally science itself, were to reap from the applications of scientific methods to the investigation of nature.

Even though the investigations of Descartes, Newton, Leibnitz, Boyle, Torricelli, and Malpighi, had directly helped no man to either wealth or comfort, the cumulative results of their labors, and those of their pupils and associates, resulted in a condition of scientific knowledge from which, sooner or later, utilitarian results must necessarily have sprung.

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* Wood, T. H.: The Reign of Victoria; a survey of Fifty Years of Progress. London, 1887.

It is true, as Huxley tells us, that at the beginning of this century weaving and spinning were still carried on with the old appliances; true that nobody could travel faster by sea or by land than at any previous time in the world's history, and true that King George could send a message from London to York no faster than King John might have done. Metals were still worked from their ores by immemorial rule of thumb, and the centre of the iron trade of these islands was among the oak forests of Sussex, while the utmost skill of the British mechanic did not get beyond the production of a coarse watch.

It cannot be denied that although the middle of the eighteenth century was illuminated by a host of great names in science, chemists, biologists, geologists, English, French, German, and Italian, the deepening and broadening of natural knowledge had produced next to no immediate practical benefits. Still I cannot believe that Bacon, the prophet, would have been so devoid of "scientific insight" as to have failed to foresee at this time the ultimate results of all this intellectual activity.

But Huxley says:

> "Even if, at this time, Francis Bacon could have returned to the scene of his greatness and of his littleness, he must have regarded the philosophic world which praised and disregarded his precepts with great disfavor. If ghosts are consistent, he would have said, "these people are all wasting their time, just as Gilbert, and Kepler, and Galileo, and my worthy physician Harvey did in my day. Where are the fruits of the restoration of science which I promised? This accumulation of bare knowledge is all very well, but *cui bono?* Not one of these people is doing what I told him specially to do, and seeking that secret of the cause of forms, which will enable him to deal at will with matter and superinduce new nature upon old foundations."

As Huxley, however, proceeds himself to show, in the discussion which immediately follows this passage, a "new nature, begotten by science upon fact," has been born within the past few decades, and pressing itself daily and hourly upon our attention, has worked miracles which have not only modified the whole future of the lives of mankind, but has reacted constantly upon the progress of science itself.

It is to the development of this new nature, then in its very infancy, that we must look for the revival of interest in science on this side of the Atlantic.

The second decade of the century was marked by a great accession of interest in the sciences. The second war with Great Britain having ended, the country, for the first time since colonial days, became sufficiently tranquil for peaceful attention to literature and philosophy. The end of the Napoleonic wars and the restoration of tranquillity to Europe tended to scientific advances on the other side of the Atlantic, and the results of the labors of Cuvier, whose glory was now approaching its zenith, of Brongniart, of Blainville, of Jussieu, of Decandolle, of Werner, of Hutton, of Buckland, of De la Beche, of Magendie, of Humboldt, Daubuisson, Berzelius, Von Buch, of Herschel, of Laplace, of Young, of Fresnel, of Oersted, of Cavendish, of Lavoisier, Wollaston, Davy, and Sir William Hooker, were eagerly welcomed by hundreds in America.

"In truth," wrote one who was among the most active in promoting these tendencies, "in truth, a thirst for the Natural Sciences seemed already to pervade the United States like the progress of an epidemic."

The author of these enthusiastic words was Amos Eaton [b. in Chatham, N. Y., 1776, d. May 6, 1842], one of the most interesting men of his day. In 1816, at the age of forty, he abandoned the practice of law and went to New Haven to attend Silliman's lectures on Mineralogy and Geology. He was a man of great force and untiring energy, and one of the pioneers of American geology; though the name, "father of American geology," sometimes applied to him, would seem to belong more appropriately to Maclure, or, perhaps, to Mitchill. He was, however, only some eight years later than Maclure in beginning geological field-work. Eaton's "Index to the Geology of the Northern States of America," printed in 1817, was the first strictly American treatise, and seems to have had a very stimulating effect. He was pre-eminently an agitator and an educator. He travelled many thousands of miles on foot throughout New England and New York, delivering, in the meantime, at the principal towns, short courses of lectures on natural history. In March, 1817, having received an invitation to aid in the introduction of the Natural Sciences in Williams College, his Alma Mater, he delivered a course of lectures in Williamstown. "Such," he remarks, "was the zeal at this institution that an uncontrollable enthusiasm for natural history took possession of every mind; and other departments of learning were, for a time, crowded out of the college. The authorities allowed twelve students each day (seventy-two per week) to devote their whole time to the collection of minerals and plants, in lieu of all other exercises."*

In April, 1818, he went to Albany on the special invitation of Gov. DeWitt Clinton and delivered a course of lectures on Natural History. "In Albany I found," wrote he, "Dr. T. Romeyn Beck, and in Troy, Doctors Burrett, Robbins, and Dale, zealous beyond description in the cause of Natural Science. By the exertions of these gentlemen a taste for the study of Nature was strongly excited in those two cities, especially for that of geology. They, together with several others, had become members of the New York Lyceum of Natural History, and, in the fall of 1818, established a society of the same name and upon a similar plan in Troy. Collections were made with such zeal that, in the course of a few months, Troy could boast of a more extensive collection of American geological specimens than Yale College, or any other institution upon this continent."*

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* Geological Text-Book, 2d ed., 1832, p. 16.

"In this period," remarked Bache, "the prosecution of mathematics and physical science was neglected; indeed barely kept alive by the calls for boundary and land surveys of the more extended class, by the exertions necessary in the lecture-room, or by isolated volunteer efforts.

"As the country was explored and settled the unworked mine of natural history was laid open, and the attention of almost all the cultivators of science was turned toward the development of its riches.

"Descriptive natural history is the pursuit which emphatically made that period. As its experiment may be taken the admirable descriptive mineralogy of Cleaveland, which seemed to fill the measures of that day and be, as it were, its chief embodiment, appearing just as the era was passing away."†

The leading spirits of the day seem to have been Silliman, Hare, Maclure, Mitchill, Gibbs, Cleaveland, DeWitt Clinton, and Caspar Wistar.

Names familiar to us of the present generation began now to appear in scientific literature: Isaac Lea began to print his memoirs on the *Unionidæ;* Edward Hitchcock, principal of the Deerfield Academy, was writing his first papers on the geology of Massachusetts; Prof. Chester Dewey, of Williams College, [b. 1781, d. 1867], afterwards known to us all from his excellent work upon the Carices, was discussing the mineralogy and geology of Massachusetts; Dr. John Torrey, also to be famous as a botanist, was then devoting his attention to mineralogy and chemistry; Dr. Jacob Porter was making botanical observations in central Massachusetts; quaint old Caleb Atwater, at that time almost the only scientific observer west of the Alleghanies, was discussing the origin of prairies, meteorology, botany, geology, mineralogy, and scenery of the Ohio country, and a little later the remains of mammoths.

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* The Troy Lyceum of Natural History was incorporated in 1819, and a lectureship was created, filled by Mr. Eaton (*Silliman's Journal*, ii, 173). In 1820 a similar association, "The Hudson Association for Improvement in Science," was founded in the city of Hudson, and in 1821 the Delaware Chemical and Geological Society.

† Presidential Address Am. Assoc. Adv. Sci., 1851, pp. vi, xlvi.

Prof. J. W. Webster, of Boston, was making general studies in geology; the Rev. Elias Cornelius and Mr. John Grammer were writing of the geology of Virginia; Mr. J. A. Kain, upon that of Tennessee, I. P. Brace, that of Connecticut, and James Pierce, that of New Jersey.

To this period belonged the brilliant Constantine Rafinesque, with Torrey, Silliman, Cleaveland, Gibbs, James, Schoolcraft, Gage, Akerly, Mitchill, Dana, Beck, and Featherstonhaugh.

Dr. Henry R. Schoolcraft, afterwards prominent in ethnology, printed, in 1819, his "View of the Lead Mines of Missouri," the first from American contributors to economic geology; and in the same year his "Transallegania," a mineralogical poem, probably the last as well as the first of its kind written in America. In 1821 he published a scholarly "Account of the Native Copper on the Southern shore of Lake Superior."*

Mineralogy and geology were the most popular of the sciences.

American Geology dated its beginning from this previous decade. Prof. S. L. Mitchill was one of the first to call attention to the teachings of Kirwan and the pioneers of European geology, and very early in the century began to instruct the students of Columbia College in the principles of geology as then understood. He published Observations on the Geology of America, and also edited a New York edition of Cuvier's "History of the Earth," contributing to this work an appendix which was constantly quoted by early writers.

The first geological explorer was William Maclure [b. in Ayr,

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* Amer. Jour. Science, iii, pp. 201–210.

Scotland, 1763, d. in San Angel, Mexico, Mar. 23, 1840], a Scotch merchant who amassed a large fortune by commercial connections with this country, and became a citizen of the United States about 1796. His most important service to American science was that of a patron, for he was a liberal supporter of the infant Academy of Sciences in Philadelphia, and for twenty-two years its president, besides being an upholder of other important enterprises.

The publication in 1809 of his "Observations on the Geology of the United States" marks the beginning of American geographical geology and the first attempt at a geological survey of the United States. This had long been the object of his ambition, and, in order to prepare himself for the task, he had spent several years in travel throughout Europe, making observations and collecting objects in natural history, which he forwarded to the country of his adoption.

His undertaking was undoubtedly a remarkable one. "He went forth with his hammer in his hand and his wallet on his shoulder, pursuing his researches in every direction, visiting almost every State and Territory, wandering often amidst pathless tracts and dreary solitudes until he had crossed and recrossed the Alleghany mountains not less than fifty times. He encountered all the privations of hunger, thirst, fatigue, and exposure, month after month and year after year, until his indomitable spirit had conquered every difficulty and crowned his enterprise with success,"* and after the publication of his memoir he devoted eight years more to collecting materials for a second and revised addition.

The geological map of the United States, published in 1809, appears to have been the first of the kind ever attempted for an entire country. Smith's geological map of England was six years later, and Greenough's still subsequent in date.

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* MARTIN: Memoir of William Maclure, p. 11.

The publication in London in 1813 of Bakewell's "Introduction to Geology" seems to have given a great stimulus to geological researches in this country, as may be judged from the publication of an American edition a year or two later.

Mitchill, Bruce, and Maclure soon had a goodly band of associates. Naturalists were not confined to limited specialties in those days, and we find all the chemists, botanists, and zoölogists absorbed in the consideration of geological problems. Maclure and most of the Americans were disciples of Werner. Silliman, writing in 1818, said:

> "A grand outline has recently been drawn by Mr. Maclure with a masterly hand and with a vast extent of personal observation and labour; but, to fill up the detail, both observation and labour still more extensive are demanded; nor can the object be effected till more good geologists are formed and distributed over our extensive territory."

On the 6th of September, 1819, the American Geological Society was organized in the philosophical room of Yale College, an event of great importance in the history of science, hastening, as it seems to have done, the establishment of State surveys and stimulating observation throughout the country. This Society, which continued in existence until about 1826, may fairly be considered the nucleus of the Association of American Geologists and Naturalists, and, consequently, of the American Association for the Advancement of Science. Members appended to their names the symbols, M. A. G. S., and it was for a time the most active of American scientific societies.

The characteristics of the leading spirits were summed up by Eaton at the time of its beginning:

> "The President, William Maclure, has already struck out the grand outline of North American geographical geology. The first Vice-President, Col. G. Gibbs, has collected more facts and amassed more geological and mineralogical specimens than any other individual of the age. The second Vice-President, Professor Silliman, gives the true scientific dress to all the naked

mineralogical subjects which are furnished to his hand. The third Vice-President, Professor Cleaveland, is successfully employed in elucidating and familiarizing those interesting scenes; and thus smoothing the rugged paths of the student. Professor Mitchill has amassed a large store of materials and annexed them to the labors of Cuvier and Jameson. The drudgery of climbing cliffs and descending into fissures and caverns, and of traversing in all directions our most rugged mountainous districts, to ascertain the distinctive characters, number, and order of our strata, has devolved upon me."*

Eaton has very fairly defined his own position among the early geologists, which was that of an explorer and pioneer. The epithet, "Father of American Geology," which has sometimes been applied to him, might more justly be bestowed upon Maclure, or even upon Mitchill. The name of Amos Eaton [b. 1776, d. 1872] will always be memorable, on account of his connection with the geological survey of New York, which was begun in 1820, at the private expense of Hon. Stephen Van Rensselaer; also as the founder, in 1824, of the Rensselaer Polytechnic Institute, the first of its class on the continent.

The State of New York was not pre-eminently prompt in establishing an official survey, but the liberality of Van Rensselaer and the energy of Eaton gave to New York the honor of attaching the names of its towns and counties to a large number of the geological formations of North America.

In these early surveys Eaton was associated with Dr. Theodore Romeyn Beck and Mr. H. Webster, naturalist and collector, one of the first being a survey of the county of Albany, under the special direction of a County Agricultural Society, followed by similar surveys of Rensselaer county and Saratoga county and others along the Erie Canal.

In July, 1818, Professor Silliman began the publication of the *American Journal of Science*, which has been for more than two-thirds of a century the most prominent register of the scientific progress of this continent. Silliman's journal succeeded, and far more than replaced, the *American Mineralogical Journal*, the earliest of American scientific periodicals, which was established in New York 1810 by Dr. Archibald Bruce, and which was discontinued after the close of the first volume, in 1814, on account of the illness and untimely death of its projector.* The *Mineralogical Journal* was not so limited in scope as in name, and was for a time the principal organ of our scientific specialists.†

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* Index to the Geology of the Northern States. 2d ed. 1820. p. viii.

We can but admire the spirit of Silliman, who remarks in the preface to the third volume:

> "It must require several years from the commencement of the work to decide the question [whether it is to be supported], and the editor (if God continues his life and health) will endeavour to prove himself neither impatient nor querulous during the time that his countrymen hold the question undecided, *whether there shall be an American Journal of Science and Arts*."

In the fall of 1822 he announced that a trial of four years had decided the point that the American public would support this journal.

Prior to the establishing of Silliman's journal, the principal organs of American science were the *Medical Repository*, commenced in 1798, of which Dr. Mitchill was the chief proprietor; the *New York Medical and Physical Journal*, conducted chiefly by Dr. Hosack; the *Boston Journal of Philosophy and the Arts*, and other similar periodicals. Our students looked chiefly, however, to the English journals—Tilloch's *Philosophical Magazine* and Nicholson's *Journal of Natural Philosophy*, and later, Thomson's *Annals of Philosophy*, the *Annales de Chimie*.

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* "No future historian of American science will fail to commemorate this work as our earliest *purely scientific* journal, supported by *original American communications*," said Silliman in his prospectus, 1817.

† The only copies of this journal known to be in existence are in the N. Y. State Library and the Harvard Library.

The American Monthly Magazine, established in 1814 by Charles Brockden Brown, was fully as much devoted to science as to literature, and an examination of this and other journals of the early portion of the century will, I think, satisfy the student that scientific subjects were more seriously considered by our ancestors than by the Americans of to-day. *The American Monthly* published elaborate reviews of technical works, such as Cleaveland's Mineralogy, and summaries of the world's progress in science, as well as the monthly proceedings of all the scientific societies in New York, and papers on systematic zoölogy and botany by Rafinesque.

In 1812 the American Antiquarian Society was established at Worcester, and before 1820, when its first volume of transactions appeared, had collected 6,000 books and "a respectable cabinet." This was a pioneer effort in ethnological science. *Archæologia Americana* contained papers by Mitchill, Atwater, and others, chiefly relating to the aboriginal population of America. The name of Isaiah Thomas, LL. D. [b. in Boston 1749, d. in Worcester 1831], the founder and first president of the society, who at his own expense erected a building for its accommodation and endowed its first researches, should be remembered with gratitude by American naturalists. He was one of the most eminent of American printers, and styled by DeWarville "the Didot of America."

In 1812 the Academy of Natural Sciences of Philadelphia was founded, the outgrowth of a social club, whose members, we are told, had no conception of the importance of the work they were undertaking when, in a spirit of burlesque, they assumed the title of an academy of learning.

In 1816 the Coast Survey, after years of discussion, was placed in action under the supervision of Hassler (who had been appointed its head as early as 1811), but, two years later, the work going on too slowly to please the Government, it was stopped.

The Linnæan Society of New England, established in Boston about this time, was the precursor of the Boston Society of Natural Science.

The publication of an American edition of Rees's Cyclopædia, in Philadelphia, was begun in 1810, and the 47th volume completed in 1824. This was an event in the history of American science, for it furnished employment and thus fostered the investigations of several eminent naturalists, among whom were Alexander Wilson, Thomas Say, and Ord; while, at the same time, it fostered a taste for science in the United States and gave currency to several rather epoch-making articles, such as Say's upon Conchology and Entomology.

Mr. Bradbury, the publisher of this Cyclopædia, was the first of a goodly company of liberal and far-seeing publishers who have done much for science in this country by their patronage of important scientific publications.

In 1817 Josiah Meigs, Commissioner of the Land Office, issued a circular to the several Registers of the Land Offices of the United States requiring them to keep daily meteorological observations, and also to report upon such phenomena as the times of the unfolding of leaves of plants and the dates of flowering, the migrations of birds and fishes, the dates of spawning of fishes, the hibernation of animals, the history of locusts and other insects in large numbers, the falling of stones and other bodies from the atmosphere, the direction of meteors, and discoveries relative to the antiquities of the country.

It does not appear that anything ever resulted from this step, but it is referred to as an indication that, seventy years ago, our Government was willing to use its civil service officials in the interest of science. A few years later the same idea was carried into effect by the Smithsonian Institution.

In those early days each of the principal cities had public museums founded and supported by private enterprise. Their proprietors were men of scientific tastes, who affiliated with the naturalists of the day and placed their collections freely at the disposal of investigators.

The earliest was the Philadelphia Museum, established by Charles Wilson Peale, and for a time housed in the building of the American Philosophical Society. In 1800 it was full of popular attractions.

> "There were a mammoth's tooth from the Ohio, and a woman's shoe from Canton; nests of the kind used to make soup of, and a Chinese fan six feet long; bits of asbestos, belts of wampum, stuffed birds and feathers from the Friendly Islands, scalps, tomahawks, and long lines of portraits of great men of the Revolutionary War. To visit the Museum, to wander through the rooms, play upon the organ, examine the rude electrical machine, and have a profile drawn by the physiognomitian, were pleasures from which no stranger to the city ever refrained."

Dr. Hare's oxyhydrogen blow-pipe was shown in this Museum by Mr. Rubens Peale as early as 1810.

The Baltimore Museum was managed by Rembrandt Peale, and was in existence as early as 1815 and as late as 1830.*

Earlier efforts were made, however, in Philadelphia. Dr. Chovet, of that city, had a collection of wax anatomical models made by him in Europe, and Prof. John Morgan, of the University of Pennsylvania, who learned his methods from the Hunters in London and Sué in Paris, was also forming such a collection before the Revolution.†

The Columbian Museum and Turrell's Museum, in Boston, are spoken of in the annals of the day, and there was a small collection in the attic of the State House in Hartford.

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* "Baltimore has a handsome museum superintended by one of the Peale family, well known for their devotion to natural science and to works of art. It is not their fault if the specimens which they are enabled to display in the latter department are very inferior to their splendid exhibitions in the former."—MRS. TROLLOPE, *Domestic Manners of the Americans.* London, 1831.

† Trans. Amer. Phil. Soc., ii, p. 366.

The Western Museum, in Cincinnati, was founded about 1815, by Robert Best, M. D., afterwards of Lexington, Ky., who seems to have been a capable collector, and who contributed matter to Godman's "American Natural History." In 1818 a society styled the Western Museum Society was organized among the citizens, which, though scarcely a scientific organization, seems to have taken a somewhat liberal and public-spirited view of what a museum should be. To the naturalist of to-day there is something refreshing in such simple appeals as the following:

> "In collecting the fishes and reptiles of the Ohio the managers will need all the aid which their fellow-citizens may feel disposed to give them. Although not a very interesting department of zoölogy, no object of the Society offers so great a prospect of novelty as that which embraces these animals.
>
> "The obscure and neglected race of insects will not be overlooked, and any specimen sufficiently perfect to be introduced into a cabinet of entomology will be thankfully received."*

Major John Eatton LeConte, U. S. A. [b. 1784, d. 1860], was a very successful student of botany and zoölogy. He published many botanical papers and contributions to descriptive zoölogy, and also in Paris, in conjunction with Boisduval, the first instalment of a work, of which he was really sole author, upon the Lepidoptera of North America.†

The elder brother, Dr. Lewis LeConte [b. 1782, d. 1838], was equally eminent as an observer, and was, for forty years, one of the most prominent naturalists in the South. On his plantation in Liberty county, Ga., he established a botanical garden and a chemical laboratory. His zoölogical manuscripts were destroyed in the burning of Columbia just at the close of the civil war, but his observations, which he was averse to publishing in his own name, were, we are told, embodied in the writings of his brother, of Stephen Elliott, of the Scotch botanist Gordon,* of Dr. William Baldwin, and others.† ‡

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*An Address to the people of the Western Country, dated Cincinnati, Sept. 15, 1818, and signed by Elijah Slack, James Findlay, William Steele, Jesse Embrees, and Daniel Drake, Managers.

† Histoire Generale et Iconographie.

Stephen Elliott, of Charleston, South Carolina [b. 1711, d. 1830], was a graduate of Yale in the class of 1791, and, while prominent in the political and financial circles of his State, found time to cultivate science. He founded in 1813 the Literary and Philosophical Society of South Carolina, and was its first president; and in 1829 was elected Professor of Natural History and Botany in the South Carolina Medical College, which he aided to establish. He published "The Botany of South Carolina and Georgia" (Charleston, 1821–27), having been assisted in its preparation by Dr. James McBride; and had an extensive museum of his own gathering. The Elliott Society of Natural History, founded in 1853, or before, and subsequently continued under the name of the Elliott Society of Science and Art, 1859–75, was named in memory of this public-spirited man.

Jacob Green [b. 1790, d. 1841], at different times professor in the College of New Jersey and in Jefferson Medical College, was one of the old school naturalists, equally at home in all of the sciences. His paper on Trilobites (1832) was our first formal contribution to invertebrate paleontology; his "Account of some new species of Salamanders,"§ one of the earliest steps in American herpetology; his "Remarks on the Unios of the United States,"‖ the beginning of studies subsequently extensively prosecuted by Lea and some other entomologists. He also wrote upon the crystallization of snow, and was the author of "Chemical

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* Loudon's Gardeners' Magazine.

† A. H. Stephens in *Johnson's Cyclopædia*, p. 1702.

‡ The LeConte family deserves a place in Galto's "Hereditary Genius." Prof. John LeConte, the physicist, and Prof. Joseph LeConte, the geologist, were sons of Dr. Lewis LeConte; while Dr. J. L. LeConte is the son of his brother, Major LeConte.

§ Contributions of the Maclurian Lyceum, i, Jan., 1827, p. 3.

‖ Ibid, i, ii, 41.

Philosophy," "Astronomical Researches," and a work upon Botany of the United States.

The earlier volumes of Silliman's Journal were filled with notes of his observations in all departments of natural history.

José Francisco Correa da Serra, secretary of the Royal Academy of Lisbon, was resident in Philadelphia in 1813, in the capacity of Portuguese minister, and affiliated with our men of science in botanical and geological interests. In 1814 he lectured on botany in the place of B. S. Barton, and also published several botanical papers, as well as one upon the soil of Kentucky.

Alire Raffenau Delile, formerly a member of Napoleon's scientific expedition to Egypt, and the editor of the "Flora of Egypt," was in New York about this time, for the purpose of completing his medical education, and seems to have done much to stimulate interest in botanical studies.

To this as well as to the subsequent period belonged Dr. Gerard Troost [b. in Holland, Mar. 15, 1776, ed. at Leyden, d. at Nashville, Aug. 17, 1850], a naturalist of Dutch birth and education, who came to Philadelphia in 1810, and was a founder and the first President of the Philadelphia Academy. In 1826 he founded a Geological Survey of the environs of Philadelphia; in 1827 became Professor of Chemistry, Mineralogy and Geology in the University of Nashville. As State geologist of Tennessee from 1831–49 he published some of the earliest State geological reports.

Another expedition, well worthy of mention, though not exceedingly fruitful, was one made under the direction of Mr. Maclure, President of the Philadelphia Academy, to the Sea Islands of Georgia and the Florida peninsula. The party consisted of Maclure, Say, Ord, and Titian R. Peale, and its results, though not embodied in a formal report, may be detected in the scientific literature of the succeeding years. This was early in 1818, while Florida was still under the dominion of

Spain, and the expedition was finally abandoned, owing to the hostile attitude of the Seminole Indians in that territory.

XIV.

The third decade of the century, beginning with 1820, was marked by a continuation of the activities of that which preceeded. In 1826 there were in existence twenty-five scientific societies, more than half of them especially devoted to natural history,* and nearly all of very recent origin.

The leading spirits were Mitchill, Maclure, Webster, Torrey, Silliman, Gibbs, LeConte, Dewey, Hare, Hitchcock, Olmstead, Eliot, and T. R. Beck.

Nathaniel Bowditch [b. 1773, d. 1838], who, in 1829, began the publication of his magnificent translation of the "Mecanique Celeste" of La Place, with those scholarly commentations which secured him so lofty a place among the mathematicians of the world.

Still more important was the lesson of his noble devotion of his life and fortune to science. The greater part of his monumental work was completed, we are told, in 1817, but he found that to print it would cost $12,000, a sum far beyond his means. A few years later, however, he began its publication from his own limited means, and the work was continued, after his death, by his wife. The dedication is to his wife, and tells us that "without her approbation the work would not have been undertaken."

Another person was W. C. Redfield [b. 1789, d. 1857], who, in 1827, promulgated the essential portions of the theory of storms, which is now pretty generally accepted, and which was subsequently extended by Sir William Reid in Barbadoes and Bermuda, and greatly modified by Professor Loomis, of New Haven. An eloquent eulogy of Redfield was pronounced by

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* Amer. Journ. Sci., x, p. 368. (Cut).

Professor Denison Olmsted at the Montreal meeting of the American Association in 1857.*

Among the rising young investigators appear the names of Joseph Henry, A. D. Bache, C. U. Shepard, the younger Silliman, Henry Seybert, William Mather, Ebenezer Emmons, Percival, the poet geologist, DeKay, Godman, and Harlan.

The organization, in 1824, of the Rensselaer School, afterwards the Rensselaer Polytechnic Institute, at Troy, marked the beginning of a new era in scientific and technological education. Its principal professors were Amos Eaton and Dr. Lewis C. Beck.

In 1820 an expedition was sent by the General Government to explore the Northwestern Territory, especially the region around the Great Lakes and the sources of the Mississippi. This was under charge of Gen. Lewis Cass, at that time Governor of Michigan Territory. Henry R. Schoolcraft accompanied this expedition as mineralogist, and Capt. D. B. Douglass, U. S. A., as topographical engineer; and both of these sent home considerable collections reported upon by the specialists of the day. Cass himself, though better known as a statesman, was a man of scientific tastes and ability, and his "Inquiries respecting the History, Traditions, Languages, &c., of the Indians," published at Detroit in 1823, is a work of high merit.

Long's expeditions into the far West were also in progress at this time, under the direction of the General Government; the first, or Rocky Mountain, exploration in 1819–20; the second to the sources of the St. Peter's, in 1823. In the first expedition Major Long was accompanied by Edwin James as botanist and geologist, who also wrote the Narrative published in 1823. The second expedition was accompanied by William H. Keating, Professor of Mineralogy and Chemistry in the University of Pennsylvania, who was its geologist and historiographer. Say was the zoölogist of both explorations. De Schweinitz worked up the botanical material which he collected.

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* See History of N. Y. Academy of Science, p. 76.

The English expeditions sent to Arctic North America under the command of Sir John Franklin were also out during these years, the first from 1819 to 1822, the second from 1825 to 1827, and yielded many important results. To naturalists they have an especial interest, because Sir John Richardson, who accompanied Franklin as surgeon and naturalist, was one of the most eminent and successful zoölogical explorers of the century, and had more to do with the development of our natural history than any other man not an American.

His natural history papers in Franklin's reports, 1823 and 1828, his "Fauna Boreali Americana," published between 1827 and 1836, his report upon the "Zoölogy of North America," are all among the classics of our zoölogical literature.*

The third decade was somewhat marked by a renewal of interest in zoölogy and botany, which had, during the few preceding years, been rather overshadowed by geology and mineralogy.

Rafinesque had retired to Kentucky, where, from his professor's chair in Transylvania University, he was issuing his *Annals of Nature* and his *Western Minerva;* and his brilliancy being dimmed by distance, other students of animals had a chance to work.

One of the most noteworthy of the workers was Thomas Say [b. 1787, d. 1834], who was a pioneer in several departments of systematic zoölogy. A kinsman of the Bartrams, he spent many of his boyhood days in the old botanic garden at Kingsessing, in company with the old naturalist, William Bartram, and the ornithologist Wilson. At the age of twenty-five, having been unsuccessful as an apothecary, he gave his whole time to zoölogy. He slept in the hall of the Academy of Natural

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* See Rev. John McIlwraith's Life of Sir John Richardson, C. B., LL. D. London, 1868. Also Obituary in *London Reader*, 1865, p. 707.

Sciences, where he made his bed beneath the skeleton of a horse, and fed himself upon bread and milk. He was wont, we are told, to regard eating as an inconvenient interruption to scientific pursuits, and to wish that he had been created with a hole in his side, through which his food might be introduced into his system. He built up the museum of the society, and made extensive contributions to biological science.

His article on conchology, published in 1816 in the American edition of Nicholson's Cyclopædia, was the foundation of that science in this country, and was republished in Philadelphia in 1819, with the title, "A Description of the Land and Fresh-water Shells of the United States."

> "This work," remarked a contemporary, "ought to be in the possession of every American lover of Natural Science. It has been quoted by *M. Lamarck* and adopted by *M. de Ferrusac*, and has thus taken its place in the scientific world."

Such was fame in America in the year of grace 1820.

In 1817 he did a similar service for systematic entomology, and his contributions to herpetology, to the study of marine invertebrates, especially the crustacea, and to that of invertebrate paleontology, were equally fundamental.

As naturalist of Long's expeditions he described many Western vertebrates, and also collected Indian vocabularies, and it is said that the narrative of the expeditions was chiefly based upon the contents of his note-books.

In 1825 he removed from Philadelphia to New Harmony, Indiana, and, in company with Maclure and Troost, became a member of the community founded there by Owen of Lanark. Comparatively little was thenceforth done by him, and we can only regret the untimely close of so brilliant a career.*

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* See Memoirs by B. H. Coates, read before American Philosophical Society, Dec. 16, 1834. Memoirs by George Ord; also a tribute to his memory in Dall's presidential address before the Society in January, 1888.

Charles Alexander Lesueur [b. at Havre-de-Grace, France, Jan. 1, 1778, d. at Havre, Dec. 12, 1846], the friend and associate of Maclure and Say, accompanied them to New Harmony. The romantic life of this talented Frenchman has been well narrated in his biography by Ord.* He was one of the staff of the Baudin expedition to Australia in 1800, and to his efforts, seconding those of Peron, his associate, were due most of the scientific results which France obtained from that ill-fated enterprise. Lesueur, though a naturalist of considerable ability, was, above all, an artist. The magnificent plates in the reports prepared by Peron † and Freycinet ‡ were all his. He was called "the Raffaelle of zoölogical painters," and his removal to America in 1815 was greatly deplored by European naturalists. He travelled for three years with Maclure, exploring the West Indies and the eastern United States, making a magnificent collection of drawings of fishes and invertebrates, and in 1818 settled in Philadelphia, where, supporting himself by giving drawing lessons, he became an active member of the Academy of Sciences, and published many papers in its Journal.

No one ever drew such exquisite figures of fishes as Lesueur, and it is greatly to be regretted that he never completed his projected work upon North American Ichthyology. He issued a prospectus, with specimen plates, of a "Memoir on the Medusæ," and his name will always be associated with the earliest American work upon marine invertebrates and invertebrate paleontology, because it was to him that Say undoubtedly owed his first acquaintance with these departments of zoölogy. In 1820, while at Albany in the service of the United States and Canadian Boundary Commission, he gave lessons to Eaton and identified his fossils, thus laying the foundations for the future work of the rising school of New York paleontologists.

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* ORD: Memoir of Charles Alexander Lesueur. *Am. Jour. Sci.*, 2d ser., viii, p. 189.

† Voyage des Decouvertes aux Terres Australes.

‡ Voyage aux Terres Australes.

Twelve years of his life were wasted at New Harmony, and in 1837, after the death of Say, he returned to France, carrying his collections and drawings to the Natural History Museum at Havre, of which he became Curator. His period of productiveness was limited to the six years of his residence in Philadelphia. But for their sacrifice to the socialistic ideas of Owen, Say and Lesueur would doubtless be counted among the most distinguished of our naturalists, and the course of American zoölogical research would have been entirely different.

The Rev. Daniel H. Barnes [b. 1785, d. 1828], of New York, a graduate of Union College and a Baptist preacher, was one of Say's earliest disciples, and from 1823 he published papers on conchology, beginning with an elaborate study of the fresh-water mussels. This group was taken up in 1827 by Dr. Isaac Lea, and discussed from year to year in his well-known series of beautifully illustrated monographs.

Mr. Barnes published, also, papers on the "Classification of the Chitonidæ," on "Batrachian Animals and Doubtful Reptiles," and on "Magnetic Polarity."

The officers of the Navy had already begun their contributions to natural history which have been so serviceable in later years. One of the earliest contributions by Barnes was a description of five species of *Chiton* collected in Peru by Capt. C. S. Ridgely, of the "Constellation."

In this period (1828+) was begun the publication of Audubon's folio volumes of illustrations of North American birds—a most extraordinary work, of which Cuvier enthusiastically exclaimed: "C'est le plus magnifique monument que l'Art ait encore élevé a la Nature."

Wilson was the Wordsworth of American naturalists, but Audubon was their Rubens. With pen as well as with brush he delineated those wonderful pictures which have been the delight of the world.

Born in 1781, in Louisiana, while it was still a Spanish colony, he became, at an early age, a pupil of the famous French painter David, under whose tuition he acquired the rudiments of his art. Returning to America, he began the career of an explorer, and for over half a century his life was spent, for the most part, in the forests or in the preparation of his ornithological publications—occasionally visiting England and France, where he had many admirers. His devotion to his work was as complete and self-sacrificing as that of Bowditch, the story of whose translation of LaPlace has already been referred to. It was a great surprise to his friends (though his own fervor did not permit him to doubt) that the sale of his folio volumes was sufficient to pay his printer's bills. Audubon was not a very accomplished systematic zoölogist, and when serious discriminations of species was necessary, sometimes formed alliances with others. Thus Bachman became his collaborator in the study of mammals, and the youthful Baird was invited by him, shortly before his death in 1851, to join him in an ornithological partnership. His relations with Alexander Wilson form the subject of a most entertaining narration in the "Ornithological Biography."*

Thomas Nuttall [b. in Yorkshire, 1786, d. at St. Helens, Lancashire, Sept. 10, 1859] was so thoroughly identified with American natural history and so entirely unconnected with that of England that, although he returned to his native land to die, we may fairly claim him as one of our own worthies. He crossed the ocean when about twenty-one years of age, and travelled in every part of the United States and in the Sandwich Islands studying birds and plants. From 1822 to 1828 he was curator and lecturer at the Harvard Botanical Garden. Besides numerous papers in the Proceedings of the Philadelphia Academy, he published in Philadelphia, in 1818, his "Genera of North

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* I, p. 439.

American Plants," in his "Geological Sketch of the Valley of the Mississippi," in 1821; his "Journal of Travels into the Arkansas Territory," a work abounding in natural history observations; in 1832–4 his "Manual of the Ornithology of the United States and Canada;" and in 1843–9 his "North American Sylva," a continuation of the Sylva of Michaux. About 1850 he retired to a rural estate in England, where he died in 1859.

Nuttall was not great as a botanist, as a geologist, or a zoölogist, but was a man useful, beloved, and respected.

Richard Harlan, M. D. [b. 1796, d. 1843], who, with Mitchill, Say, Rafinesque, and Gosse, was one of the earliest of our herpetologists, and who was one of Audubon's chief friends and supporters, published in 1825 the first instalment of his "Fauna Americana," which treated exclusively of mammals. This was followed, in 1826, by a rival work on mammals, by Godman. Harlan's book was a compilation, based largely on translations of portions of Desmarest's "Mammalogie," printed three years before in Paris. It was so severely criticised that the second portion, which was to have been devoted to reptiles, was never published, and its author turned his attention to medical literature. Godman's "North American Natural History, or Mastology," contained much original matter, and, though his contemporaries received it with faint praise, it is the only separate, compact, illustrated treatise on the mammals of North America ever published, and is useful to the present day. John D. Godman [b. in Annapolis, Md., Dec. 20, 1794, d. in Germantown, Pa., Apl. 17, 1830] died an untimely death, but gave promise of a brilliant and useful career as a teacher and investigator. His "Rambles of a Naturalist" is one of the best series of essays of the Selborne type ever produced by an American, and his "American Natural History" is a work of much importance, even to the present day, embodying as it does a large number of original observations.

Michaux's Sylva was, as we have seen, continued by Nuttall:

Wilson's American Ornithology was, in like manner, continued by Charles Lucien Bonaparte [b. in Paris, May 24, 1803, d. in Paris, July 30, 1857], Prince of Canino, and nephew of Napoleon the First, a master in systematic zoölogy. Bonaparte came to the United States about the year 1822, and returned to Italy in 1828. His contributions to zoölogical science were of great importance. In 1827, he published in Pisa his "Specchio comparativo delle ornithologie di Roma e di Filadelfia," and from 1825 to 1833 his "American Ornithology," containing descriptions of over one hundred species of birds discovered by himself.

The publication of Torrey's "Flora of the Middle and Northern Sections of the United States" was an event of importance, as was also Dr. W. J. Hooker's essay on the Botany of America,* the first general treatise upon the American flora or fauna, by a master abroad, is pretty sure evidence that the work of home naturalists was beginning to tell.

So, also, in a different way, was the appearance in 1829 of the first edition of Mrs. Lincoln's "Familiar Lectures on Botany," a work which did much toward swelling the army of amateur botanists.

Important work was also in progress in geology. Eaton and Beck were carrying on the Van Rensselaer survey of New York, and in 1818 the former published his "Index to the Geology of the Northern States." Prof. Denison Olmstead, of the University of North Carolina, was completing the official survey of that State—the first ever authorized by the government of a State.

Prof. Lardner Vanuxem, of North Carolina, in 1828, made an important advance, being the first to avail himself successfully of paleontology for the determination of the age of several of our formations, and their approximate synchronism with European beds.†

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* Brewster's Edinburgh Journal of Science, iii, p. 103.

† Gill.

Horace H. Hayden, of Baltimore [b. 1769, d. 1844], published in 1820 "Geological Essays, or an inquiry into some of the geological phenomena to be found in various parts of America and elsewhere,"* which was well received as a contribution to the history of alluvial formations of the globe, and was apparently the first general work on geology published in this country. Silliman said that it should be a text-book in all the schools. He published, also, a "New Method of preserving Anatomical Preparations,"† "A Singular ore of Cobalt and Manganese,"‡ on "The Bare Hills near Baltimore,"‖ and on "Silk Cocoons,"§ and was a founder and vice-president of the Maryland Academy of Sciences.

XV.

In the fourth decade (1830–40) the leading spirits were Silliman, Hare, Olmstead, Hitchcock, Torrey, DeKay, Henry, and Morse.

Among the men just coming into prominence were J. W. Draper, then professor in Hampden Sidney College, in Virginia, the brothers W. B. and H. D. Rogers, A. A. Gould the conchologist, and James D. Dana.

Henry was just making his first discoveries in physics, having, in 1829, pointed out the possibility of electro-magnetism as a motive power, and in 1831 set up his first telegraphic circuit at Albany. In 1832 the United States Coast Survey, discontinued in 1818, was reorganized under the direction of its first chief, Hassler, now advanced in years.¶

The natural history survey of New York was organized by the

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* Rev. Sill. Journ., iii, 47. Blackwood's Mag., xvi, 420; xvii, 56.

† American Medical Record, 1822.

‡ *Ibid.* 1832. ‖ Silliman's Journal, 1822.

§ Journ. Amer. Silk Company, 1839.

¶ Proc. Amer. Assoc. Adv. Sci., ii, 163.

State in 1836, and James Hall and Ebenezer Emmons were placed upon its staff.

G. W. Featherstonhaugh [b. 1780, d. 1866] was conducting (1834–5) a Government expedition, exploring the geology of the elevated country between the Missouri and Red rivers and the Wisconsin territories. He bore the name of "United States Geologist," and projected a geological map of the United States, which now, half a century later, is being completed by the U. S. geologist of to-day. Besides his report upon the survey just referred to, Featherstonhaugh printed a "Geological Reconnoissance, in 1835, from Green Bay to Coteau des Prairies," and a "Canoe Voyage up the Minnay Sotor," in London, 1847.

In 1838 the United States Exploring Expedition under Wilkes was sent upon its voyage of circumnavigation, having upon its staff a young naturalist named Dana, whose studies upon the crustaceans and radiates of the expedition have made him a world-wide reputation, entirely independent of that which he has since gained as a mineralogist and geologist. It is customary to refer to the Wilkes expedition as having been sent out entirely in the interests of science. As a matter of fact it was organized primarily in the interests of the whale fishery of the United States.

Dana, before his departure with Wilkes, had published, in 1837, the first edition of his "System of Mineralogy," a work which, in its subsequent editions, has become the standard manual of the world.

The publication of Lyell's "Principles of Geology" at the beginning of this decade (1830) had given new direction to the thoughts of our geologists, and they were all hard at work under its inspiration.

With 1839 ended the second of our thirty-year periods—the one which I have chosen to speak of as the period of Silliman—not so much because of the investigations of the New Haven professor, as on account of his influence in the promotion of American Science and scientific institutions.

This was a time of hard work, and we must not withhold our praise from the noble little company of pioneers who were, in those years, building the foundations upon which the scientific institutions of to-day are resting.

The difficulties and drawbacks of scientific research at this time have been well described by one who knew them :*

> " The professedly scientific institutions of our country issued, from time to time, though at considerable intervals, volumes of transactions and proceedings unquestionably not without their influence in keeping alive the scarcely kindled flame, but whose contents, as might be expected, were, for the most part, rather in conformity with the then existing standard of excellence than in advance of it. Natural history in the United States was the mere sorting of genera and species. The highest requisite for distinction in any physical science was the knowledge of what European students had attained. Astronomy was, in general, confined to observations, and those not of the most refined character, and its merely descriptive departments were estimated far more highly than the study of its laws. Astronomical computation had hardly risen above the ciphering out of eclipses and occultations. Indeed, I risk nothing in saying that astronomy had lost ground in America since those colonial times, when men like Rittenhouse kept up a constant scientific communication with students of astronomy beyond the seas. And I believe I may farther say, that a single instance of a man's devoting himself to science as the only earthly guide, aim, and object of his life, while unassured of a professor's chair or some analogous appointment upon which he might depend for subsistence, was utterly unknown.
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> " Such was the state of science in general. In astronomy the expensive appliances requisite for all observations of the higher class were wanting, and there was not in the United States, with the exception of the Hudson Observatory, to which Professor Loomis devoted such hours as he could spare from his duties in the college, a single establishment provided with the means of making an absolute determination of the place of any celestial body, or even relative determinations at all commensurate in accuracy with the demands of the times. The only instrument that could be thought of for the purpose was the Yale College telescope, which, although provided with a micrometer, was destitute of the means of identifying comparison-stars. A better idea of American astronomy a dozen years ago can hardly be obtained than by quot-

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* Gould, B. A. Address in commemoration of Sears Cook Walker. <*Proc. Amer. Assoc. Ad. Sci.*, viii, 25

ing from an article published at that time by the eminent geometer who now retires from the position of President of this Association. He will forgive me the liberty for the sake of the illustration. 'The impossibility,' said he, 'of great national progress in astronomy, while the materials are, for the most part, imported, can hardly need to be impressed upon the patrons of science in this country. * * * And next to the support of observers is the establishment of observatories. Something has been done for this purpose in various parts of the country, and it is earnestly to be hoped that the intimations which we have heard regarding the intentions of Government may prove to be well founded; that we shall soon have a permanent national observatory equal in its appointments to the best furnished ones of Europe; and that American ships will ere long calculate their longitudes and latitudes from an American nautical almanac. That there is on this side of the Atlantic a sufficient capacity for celestial observations is amply attested by the success which has attended the efforts, necessarily humble which have hitherto been made.'"*

XVI.

Just before the middle of the century a wave, or to speak more accurately, a series of waves of intellectual activity began to pass over Europe and America. There was a renaissance, quite as important as that which occurred in Europe at the close of the Middle Ages. Draper and other historians have pointed out the causes of this movement, prominent among which were the introduction of steam and electricity, annihilating space and relieving mankind from a great burden of mechanical drudgery. It was the beginning of the "age of science," and political as well as social and industrial changes followed in rapid succession.

In Europe the great work began a little earlier. Professor Huxley, in his address to the Royal Society in 1885, took for a fixed point his own birthday in 1825, which was four months before the completion of the railway between Stockton and Darlington—"the ancestral representative of the vast reticulated fetching and carrying organism which now extends its meshes over the civilized world." Since then, he remarked, "the greater part of the vast body of knowledge which constitutes the modern sciences of physics, chemistry, biology, and geology has been acquired, and the widest generalizations therefrom have been deduced, and, furthermore, the majority of those applications of scientific knowledge to practical ends which have brought about the most striking differences between our present civilization and that of antiquity have been made within that period of time."

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* Peirce, Benjamin, Cambridge Miscellany, 1842, p. 25.

It is within the past half century, he continued, that the most brilliant additions have been made to fact and theory and serviceable hypothesis in the region of pure science, for within this time falls the establishment on a safe basis of the greatest of all the generalizations of science, the doctrines of the Conservation of Energy and of Evolution. Within this time the larger moiety of our knowledge of light, heat, electricity, and magnetism has been acquired. Our present chemistry has been, in great part, created, while the whole science has been remodelled from foundation to roof.

"It may be natural," continued Professor Huxley, "that progress should appear most striking to me among those sciences to which my own attention has been directed, but I do not think this will wholly account for the apparent advance 'by leaps and bounds' of the biological sciences within my recollection. The cell theory was the latest novelty when I began to work with the microscope, and I have watched the building of the whole vast fabric of histology. I can say almost as much of embryology, since Von Baer's great work was published in 1828. Our knowledge of the morphology of the lower plants and animals and a great deal of that of the higher forms has very largely been obtained in my time; while physiology has been put upon a totally new foundation, and, as it were, reconstructed, by the thorough application of the experimental method to the study of the phenomena of life, and by the accurate determination of the purely physical and chemical components of these phenomena.

The exact nature of the processes of sexual and non-sexual reproduction has been brought to light. Our knowledge of geographical and geological distribution and of the extinct forms of life has been increased a hundredfold, As for the progress of geological science, what more need be said than that the first volume of Lyell's 'Principles' bears the date of 1830."

It cannot be expected that, within the limits of this address, I should attempt to show what America has done in the last half century. I am striving to trace the beginnings, not the results, of scientific work on this side of the Atlantic. I will simply quote what was said by the London *Times* in 1876:

> "In the natural distribution of subjects, the history of enterprise, discovery, and conquest, and the growth of republics, fell to America, and she has dealt nobly with them. In the wider and more multifarious provinces of art and science she runs neck and neck with the mother country and is never left behind."

It is difficult to determine exactly the year when the first waves of this renaissance reached the shores of America. Silliman, in his Priestley address, placed the date at 1845. I should rather say 1840, when the first national scientific association was organized, although signs of awakening may be detected even before the beginning of the previous decade. We must, however, carefully avoid giving too much prominence to the influence of individuals. I have spoken of this period of thirty years as the period of Agassiz. Agassiz, however, did not bring the waves with him; he came in on the crest of one of them; he was not the founder of modern American natural history, but, as a public teacher and organizer of institutions, he exerted a most important influence upon its growth.

One of the leading events of the decade was the reorganization of the Coast Survey in 1844, under the sage administration of Alexander Dallas Bache,* speedily followed by the beginning of investigations upon the Gulf Stream, and of the researches of Count Pourtales into its fauna, which laid the foundations of modern deep-sea exploration. Others were the founding of the Lawrence Scientific School, the Cincinnati Observatory, the Yale Analytical Laboratory, the celebration of the Centennial Jubilee of the American Philosophical Society in 1843, and the enlargement of Silliman's "American Journal of Science."

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* Proc. Amer. Assoc. Adv. Sci., ii, 164.

The Naval Astronomical Expedition was sent to Chili, under Gibbon (1849), to make observations upon the parallax of the sun. Lieut. Lynch was sent to Palestine (in 1848) at the head of an expedition to explore the Jordan and the Dead Sea.

Frémont conducted expeditions, in 1848, to explore the Rocky Mountains and the territory beyond, and Stansbury, in 1849–'50, a similar exploration of the valley of the Great Salt Lake. David Dale Owen was heading a Government Geological Survey in Wisconsin, Iowa, and Minnesota (1848), and from all of these came results of importance to science and to natural history.

In 1849, Prof. W. H. Harvey, of Dublin, visited America and collected materials for his *Nereis Boreali-Americana*, which was the foundation of our marine botany.

Sir Charles Lyell, ex-President of the Geological Society of London, visited the United States in 1841 and again in 1845, and published two volumes of travels, which were, however, of much less importance than the effects of his encouraging presence upon the rising school of American geologists. His "Principles of Geology," as has already been said, was an epoch-making work, and he was to his generation almost what Darwin was to the one which followed.

Certain successes of our astronomers and physicists had a bearing upon the progress of American science in all its departments, which was, perhaps, even greater than their actual importance would seem to warrant. These were the discovery, by the Bards of Cambridge, of Bards comet in 1846, of the satellite Hyperion in 1848, of the third ring of Saturn in 1850, the discovery by Herrick and Bradley, in 1846, of the bi-partition of Belas comet, and the application of the telegraph to longitude determination after Locke had constructed, in 1848, his clock for the registration of time observations by means of electro-magnetism.

It is almost ludicrous at this day to observe the grateful sentiments with which our men of science welcomed the adoption of this American method in the observatory at Greenwich.

Americans were still writhing under the sting of Sidney Smith's demand "Who reads an American book?" and the narrations of those critical observers of national customs, Dickens, Basil Hall, and Mrs. Trollope.

The continental approval of American science was like balsam to the sensitive spirits of our countrymen.

John William Draper's versatile and original researches in physics were also yielding weighty results, and as early as 1847 he had already laid the foundations of the science of spectroscopy which Kirchhoff so boldly appropriated many years later.

Most important of all, by reason of its breadth of scope, was the foundation of the Smithsonian Institution, which was organized in 1846 by the election of Joseph Henry to its secretaryship. Who can attempt to say what the conditions of science in the United States would be to-day, but for the bequest of Smithson? In the words of John Quincy Adams, "Of all the foundations or establishments for pious or charitable uses which ever signalized the spirit of the age or the comprehensive beneficence of the founder, none can be named more deserving the approbation of mankind."

Among the leaders of this new enterprise and of the scientific activities of the day may be named: Silliman, Hare, Henry, Bache, Maury, Alexander, Locke, Mitchel, Peirce, Walker, Draper, Dana, Wyman, Agassiz, Gray, Torrey, Haldeman, Morton, Holbrook, Gibbes, Gould, DeKay, Storer, Hitchcock, Redfield, the brothers Rogers, Jackson, Hays, and Owen.

Among the rising men were Baird, Adams the conchologist, Burnett, Harris the entomologist, and the LeConte brothers among zoölogists; Lapham, D. C. Eaton, and Grant, among botanists; Sterry Hunt, Brush, J. D. Whitney, Wolcott Gibbs, and Lesley, among chemists and geologists, as well as Schiel, of St. Louis, who had before 1842 discovered the principle of chemical homology.

I have not time to say what ought to be said of the coming of Agassiz in 1846. He lives in the hearts of his adopted countrymen. He has a colossal monument in the museum which he reared, and a still greater one in the lives and works of pupils such as Agassiz, Allen, Burgess, Burnett, Brooks, Clarke, Cooke, Faxon, Fewkes, Gorman, Hartt, Hyatt, Joseph LeConte, Lyman, McCrady, Morse, Mills, Niles, Packard, Putnam, Scudder, St. John, Shaler, Verrill, Wilder, and David A. Wells.

XVII.

They were glorious men who represented American science at the middle of the century. We may well wonder whether the present decade will make as good a showing forty years hence.

The next decade was its continuation. The old leaders were nearly all active, and to their ranks were added many more.

An army of new men was rising up.

It was a period of great explorations, for the frontier of the United States was sweeping westward, and there was need of a better knowledge of the public domain.

Sitgreaves explored the region of the Zuñi and Colorado rivers in 1852, and Marcy the Red River of the North. The Mexican boundary survey, under Emory, was in progress from 1854 to 1856, and at the same time the various Pacific railroad surveys. There was also the Herndon exploration of the valley of the Amazon, and the North Pacific exploring expedition under Rogers. These were the days, too, when that extensive exploration of British North America was begun, through the co-operation of the Hudson's Bay Company with the Smithsonian Institution.

It was the harvest-time of the museums. Agassiz was building up with immense rapidity his collections in Cambridge, utilizing to the fullest extent the methods which he had learned in the great European establishments and the public spirit and generosity of the Americans. Baird was using his matchless powers of organization in equipping and inspiring the officers of the various surveys, and accumulating immense collections to be used in the interest of the future National Museum.

Systematic natural history advanced with rapid strides. The magnificent folio reports of the Wilkes expedition were now being published, and some of them, particularly those by Dana on the crustaceans and the zoöphytes and geology, that of Gould upon the mollusks, those by Torrey, Gray, and Eaton upon the plants, were of great importance.

The reports of the domestic surveys contained numerous papers upon systematic natural history, prepared under the direction of Baird, assisted by Girard, Gill, Cassin, Suckley, LeConte, Cooper, and others. The volumes relating to the mammals and the birds, prepared by Baird's own pen, were the first exhaustive treatises upon the mammalogy and ornithology of the United States.

The American Association was doing a great work in popular education through its system of meeting each year in a different city. In 1850 it met in Charleston, and its entire expenses were paid by the city corporation as a valid mark of public approval, while the foundation of the Charleston museum of natural history was one of the direct results of the meeting.

In 1857 it met in Montreal, and delegates from the English scientific societies were present; this was one of the earliest of those manifestations of international courtesy upon scientific ground of which there have since been many.

In the seventh decade, which began with threatenings of civil war, the growth of science was almost arrested. A meeting of the American Association was to have been held in Nashville in 1861, but none was called. In 1866, at Buffalo, its sessions were resumed with the old board of officers elected in 1860. One of the vice-presidents, Gibbes, of South Carolina, had not been heard from since the war began, and the Southern members were all absent. Many of the Northern members wrote, explaining that they could not attend this meeting because they could not afford it, "such had been the increase of living expenses, without a corresponding increase in the salaries of men of science." Few scientists were engaged in the war, though one, O. M. Mitchel, who left the directorship of the Dudley observatory to accept the command of an Ohio brigade, died in service in 1862, and another, Couthouy, sacrificed his life in the navy. Others, like Ordway, left the ranks of science never to resume their places as investigators.

Scientific effort was paralyzed, and attention was directed to other matters. In 1864, when the Smithsonian building was burned, Lincoln, it is said, looking at the flames from the windows of the Executive Mansion, remarked to some military officers who were present: "Gentlemen, yonder is a national calamity. We have no time to think about it now. We must attend to other things."

The only important events during the war were two; one the organization of the National Academy of Sciences, which soon became what Bache had remarked the necessity for in 1851, when he said: "An institution of science, supplementary to existing ones, is much needed to guide public action in reference to scientific matters."*

The other was the passage, in 1862, of the bill for the establishment of scientific educational institutions in every State.

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* Proc. Amer. Assoc. Adv. Sci., vi, xlviii.

The agricultural colleges were then, as they still are, unpopular among many scientific men, but the wisdom of the measure is apparently before long to be justified.

Before the end of the decade, the Northern States* had begun a career of renewed prosperity, and the scientific institutions were reorganized. The leading spirits were such men as Pierce, Henry, Agassiz, Gray, Barnard, the Goulds, Newberry, Lea, Whittlesey, Foster, Rood, Cooke, Newcomb, Newton, Wyman, Winchell.

Among the rising men, some of them very prominent before 1870, were Barker, Bolton, Chandler, Eggleston, Hall, Harkness, Langley, Mayer, Pickering, Young, Powell, Pumpelly, Abbe, Collett, Emerson, Hartt, Lupton, Marsh, Whitfield, Williams, N. H. Winchell, Agassiz, the Allens, Beale, Cope, Coues, Canby, Dall, Hoy, Hyatt, Morse, Orton, Perkins, Rey, Riley, Scudder, Sidney Smith, Stearns, Tuttle, Verrill, Wood.

Soon after the war the surveys of the West, which have coalesced to form the U. S. Geological Survey, were forming under the direction of Clarence Cook, Lieut. Wheeler, F. V. Hayden, and Major Powell.

The discovery of the nature of the corona of the sun by Young and Harkness in 1869 was an event encouraging to the rising spirits of our workers.

XVIII.

With 1869 we reach the end of the third period and the threshold of that in which we are living. I shall not attempt to define the characteristics of the natural history of to-day, though I wish to direct attention to certain tendencies and conditions which exist. Let me, however, refer once more to the past, since it leads again directly up to the present.

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* See A. D. WHITE's Scientific and Industrial Education in the United States. < Popular Science Monthly, v, p. 170.

In a retrospect published in 1876,* one of our leaders stated that American science during the first forty years of the present century was in "a state of general lethargy, broken now and then by the activity of some first-class man, which, however, commonly ceased to be directed into purely scientific channels." This depiction was, no doubt, somewhat true of the physical and mathematical sciences concerned, but not to the extent indicated by the writer quoted. What could be more unjust to the men of the last generation than this? "It is," continues he, "strikingly illustrative of the absence of everything like an effective national pride in science that two generations should have passed without America having produced anything to continue the philosophical researches of Franklin."

I may not presume to criticise the opinion of the writer from whom these words are quoted, but I cannot resist the temptation to repeat a paragraph from Prof. John W. Draper's eloquent centennial address upon "Science in America:"

> "In many of the addresses on the centennial occasion," he said, "the shortcomings of the United States in extending the boundaries of scientific knowledge, especially in the physical and chemical departments, have been set forth. 'We must acknowledge with shame our inferiority to other people,' says one. 'We have done nothing,' says another. * * * But we must not forget that many of these humiliating accusations are made by persons who are not of authority in the matter; who, because they are *ignorant* of what has been done, *think* that nothing *has* been done. They mistake what is merely a blank in their own information for a blank in reality. In their alacrity to depreciate the merit of their own country they would have us confess that, for the last century, we have been living on the reputation of Franklin and his thunder-rod."

These are the words of one who, himself an Englishman by birth, could, with excellent grace, upbraid our countrymen for their lack of patriotism.

The early American naturalists have been reproached for devoting their time to explorations and descriptive natural history, and their work depreciated, as being of a character beneath the dignity of the biologists of to-day.

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* North American Review.

> "The zoölogical science of the country," said the president of the Natural History Section of the American Association a few years since, "presents itself in two distinct periods: The first period may be recognized as embracing the lowest stages of the science; it included, among others, a class of men who busied themselves in taking an inventory of the animals of the country, an important and necessary work to be compared to that of the hewers and diggers who first settle a new country, but in their work demanded no deep knowledge or breadth of view."

It is quite unnecessary to defend systematic zoölogy from such slurs as this, nor do I believe that the writer quoted would really defend the ideas which his words seem to convey, although, as Professor Judd has regretfully confessed in his recent address before the Geological Society of London, systematic zoölogists and botanists have become somewhat rare and out of fashion in Europe in modern times.

The best vindication of the wisdom of our early writers will be, I think, the presentation of a counter-quotation from another presidential address, that of the venerable Dr. Bentham before the Linnæan Society of London, in 1867:

> "It is scarcely half a century," wrote Bentham, "since our American brethren applied themselves in earnest to the investigation of the natural productions and physical condition of their vast continent; their progress, especially during the latter half of that period, had been very rapid until the outbreak of the recent war, so deplorable in its effects in the interests of science as well as on the material prosperity of their country. The peculiar condition of the North American Continent requires imperatively that its physical and biological statistics should be accurately collected and authentically recorded, and that this should be speedily done. It is more than any country, except our Australian colonies, in a state of transition. Vast tracts of land are still in what may be called almost a primitive state, unmodified by the effects of civilization, uninhabited, or tenanted only by the remnants of ancient tribes, whose unsettled life never exercised

much influence over the natural productions of the country. But this state of things is rapidly passing away; the invasion and steady progress of a civilized population, whilst changing generally the face of nature, is obliterating many of the evidences of a former state of things. It may be true that the call for recording the traces of previous conditions may be particularly strong in Ethnology and Archæology; but in our own branches of the science, the observations and consequent theories of Darwin having called special attention to the history of species, it becomes particularly important that accurate biological statistics should be obtained for future comparison in those countries where the circumstances influencing those conditions are the most rapidly changing. The larger races of wild animals are dwindling down, like the aboriginal inhabitants, under the deadly influence of civilized man. Myriads of the lower orders of animal life, as well as of plants, disappear with the destruction of forests, the drainage of swamps, and the gradual spread of cultivation, and their places are occupied by foreign invaders. Other races, no doubt, without actually disappearing, undergo a gradual change under the new order of things, which, if perceptible only in the course of successive generations, require so much the more for future proof an accurate record of their state in the still unsettled condition of the country. In the Old World almost every attempt to compare the present state of vegetation or animal life with that which existed in uncivilized times is in a great measure frustrated by the absolute want of evidence as to that former state; but in North America the change is going forward, as it were, close under the eye of the observer. This consideration may one day give great value to the reports of the naturalist sent by the Government, as we have seen, at the instigation of the Smithsonian Institution and other promoters of science, to accompany the surveys of new territories."

Having said this much in defence of the scientific men of the United States, I wish, in conclusion, to prefer some very serious charges against the country at large, or, rather, as a citizen of the United States, to make some very melancholy and humiliating confessions.

The present century is often spoken of as "the age of science," and Americans are somewhat disposed to be proud of the manner in which scientific institutions are fostered and scientific investigators encouraged on this side of the Atlantic.

Our countrymen have made very important advances in many departments of research. We have a few admirably organized laboratories and observatories, a few good collections of scientific books, six or eight museums worthy of the name, and a score or more of scientific and technological schools, well organized and better provided with officers than with money. We have several strong scientific societies, no one of which, however, publishes transactions worthy of its own standing and the collective reputation of its members. In fact, the combined publishing funds of all our societies would not pay for the annual issue of a volume of memoirs, such as appears under the auspices of any one of a dozen European societies which might be named.

Our Government, by a liberal support of its scientific departments, has done much to atone for the really feeble manner in which local institutions have been maintained. The Coast Survey, the Geological Surveys, the Department of Agriculture, the Fish Commissions, the Army, with its Meteorological Bureau, its Medical Museum and Library, and its explorations; the Navy, with its Observatory, its laboratories and its explorations; and in addition to these, the Smithsonian Institution, with its systematic promotion of all good works in science, have accomplished more than is ordinarily placed to their credit. Many hundreds of volumes of scientific memoirs have been issued from the Government printing office since 1870, and these have been distributed in such a generous and far-reaching way that they have not failed to reach every town and village in the United States where a roof has been provided to protect them.

It may be that some one will accuse the Government of having usurped the work of the private publisher. Very little of value in the way of scientific literature has been issued during the same period by publishers, except in reprints or translations of works of foreign investigators. It should be borne in mind, however, that our Government has not only published the results of investigations, but has supported the investigators and provided them with laboratories, instruments and material, and that the memoirs which it has issued would never, as a rule, have been accepted by private publishers.

I do not wish to underrate the efficiency of American men of science, nor the enthusiasm with which many public men and capitalists have promoted our scientific institutions. Our countrymen have had wonderful successes in many directions. They have borne their share in the battle of science against the unknown. They have had abundant recognition from their fellow-workers in the Old World. They have met perhaps a more intelligent appreciation abroad than at home. It is the absence of home appreciation that causes us very much foreboding for the future.

In Boston or Cambridge, in New York, Philadelphia, Baltimore, Washington, Chicago, or San Francisco, and in most of the college towns, a man interested in science may find others ready to talk over with him a new scientific book, or a discovery which has excited his interest. Elsewhere, the chances are, he will have to keep his thoughts to himself. One may quickly recite the names of the towns and cities in which may be found ten or more people whose knowledge of any science is aught than vague and rudimentary. Let me illustrate my idea by supposing that every inhabitant of the United States, over fifteen years of age, should be required to mention ten living men eminent in scientific work, would one out of a hundred be able to respond? Does any one suppose that there are three or four hundred thousand people enlightened to this degree?

Let us look at some statistics, or, rather, some facts, which it is convenient to arrange in statistical form. The total number of white inhabitants of the United States in 1880 was about forty-two millions. The total number of naturalists, as shown in the *Naturalist's Directory* for 1886, was a little over 4,600. This list includes not only the investigators, who probably do not exceed five hundred in number, and the advanced teachers, who muster, perhaps, one thousand strong, but all who are sufficiently interested in science to have selected special lines of study.

We have, then, one person interested in science to about ten thousand inhabitants. But the leaven of science is not evenly distributed through the national loaf. It is the tendency of scientific men to congregate together. In Washington, for instance, there is one scientific man to every 500 inhabitants, in Cambridge one to 850, and in New Haven one to 1,100. In New Orleans the proportion is one to 8,800, in Jersey City one to 24,000, in New York one to 7,000, and in Brooklyn one to 8,500. I have before me the proportions worked out for the seventy-five principal cities of the United States. The showing is suggestive, though no doubt in some instances misleading. The tendency to gregariousness on the part of scientific men may, perhaps, be further illustrated by a reference to certain societies. The membership of the National Academy of Sciences is almost entirely concentrated about Boston, New York, Philadelphia, Washington and New Haven. Missouri has one member, Illinois one, Ohio one, Maryland, New Jersey and Rhode Island three, and California four—while thirty-two States and Territories are not represented. A precisely similar distribution of members is found in the American Society of Naturalists. A majority of the members of the American Association for the Advancement of Science live in New York, Massachusetts, Pennsylvania, the District of Columbia, Michigan, Minnesota, Ohio, Illinois and New Jersey.

It has been stated that the average proportion of scientific men to the population at large is one to ten thousand. A more minute examination shows that while fifteen of the States and Territories have more than the average proportion of scientific men, thirty-two have less. Oregon and California, Michigan and Delaware have very nearly the normal number. Massachusetts, Rhode Island, Connecticut, Illinois, Colorado and Florida have about one to four thousand. West Virginia, Nevada, Arkansas, Mississippi, Georgia, Kentucky, Texas, Alabama and the Carolinas are the ones least liberally furnished. Certain cities appear to be absolutely without scientific men. The worst cases of destitution seem to be Paterson, New Jersey, a city of 50,000 inhabitants, Wheeling, with 30,000, Quincy, Illinois, with 26,000, Newport, Kentucky, with 20,000, Williamsport, Pennsylvania, and Kingston, New York, with 18,000, Council Bluffs, Iowa, and Zanesville, Ohio, with 17,000, Oshkosh and Sandusky, with 15,000, Lincoln, Rhode Island, Norwalk, Connecticut, and Brockton and Pittsfield, Massachusetts, with 13,000. In these there are no men of science recorded, and eight cities of more than 15,000 inhabitants have only one, namely, Omaha, Nebraska, and St. Joseph, Missouri, Chelsea, Massachusetts, Cohoes, New York, Sacramento, California, Binghamton, New York, Portland, Oregon, and Leadville, Colorado.

Of course these statistical statements are not properly statistics. I have no doubt that some of these cities are misrepresented in what has been said. This much, however, is probably true, that not one of them has a scientific society, a museum, a school of science, or a sufficient number of scientific men to insure even the occasional delivery of a course of scientific lectures.

Studying the distribution of scientific societies, we find that there are fourteen States and Territories in which there are no scientific societies whatever. There are fourteen States which have State academies of science or societies which are so organized as to be equivalent to State academies.

Perhaps the most discouraging feature of all is the diminutive circulation of scientific periodicals. In addition to a certain number of specialists' journals, we have in the United States three which are wide enough in scope to be necessary to all who attempt to keep an abstract of the progress of science. Of these, the *American Journal of Science* has, we are told, a circulation of less than 800; the *American Naturalist*, less than 1,100, and *Science*, less than 6,000. A considerable proportion of the copies printed go, as a matter of course, to public institutions, and not to individuals. Even the *Popular Science Monthly* and the *Scientific American*, which appeal to large classes of unscientific readers, have circulations absurdly small.

The most effective agents for the dissemination of scientific intelligence are, probably, the religious journals, aided to some extent by the agricultural journals, and to a very limited degree by the weekly and daily newspapers. It is much to be regretted that several influential journals, which ten or fifteen years ago gave attention to the publication of trustworthy scientific intelligence, have of late almost entirely abandoned the effort. The allusions to science in the majority of our newspapers are singularly inaccurate and unscholarly, and too often science is referred to only when some of its achievements offer opportunity for witticism.

The statements which I have just made may, as I have said, prove, in some instances erroneous, and, to some extent, misleading, but I think the general tendency of a careful study of the distribution of scientific men and institutions is to show that the people of the United States, except in so far as they sanction by their approval the work of the scientific departments of the Government, and the institutions established by private munificence, have little reason to be proud of the national attitude toward science.

I am, however, by no means despondent for the future. The importance of scientific work is thoroughly appreciated, and it is well understood that many important public duties can be performed properly only by trained men of science. The claims of science to a prominent place in every educational plan are every year more fully conceded. Science is permeating the theory and the practice of every art and every industry, as well as every department of learning. The greatest danger to science is, perhaps, the fact that all who have studied at all within the last quarter of a century have studied its rudiments and feel competent to employ its methods and its language, and to form judgments on the merits of current work.

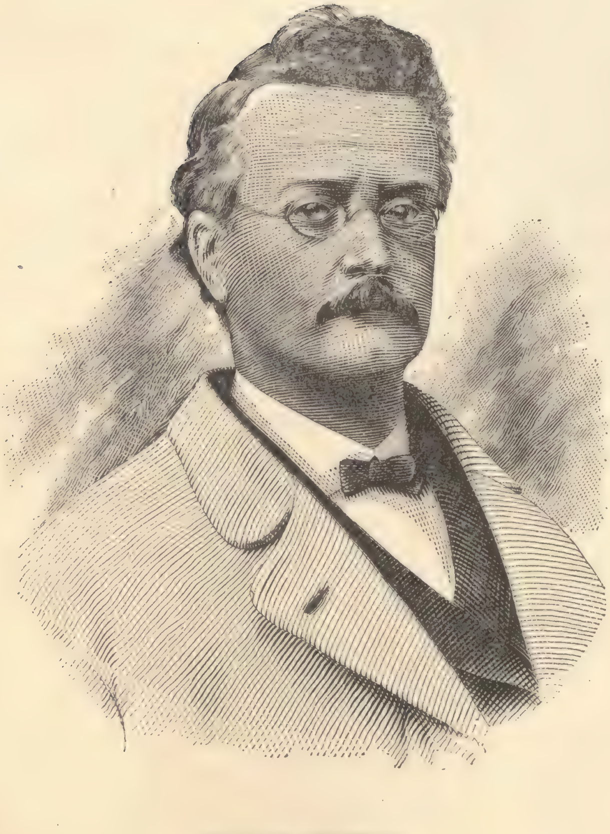
In the meantime the professional men of science, the scholars, and the investigators seem to me to be strangely indifferent to the questions as to how the public at large is to be made familiar with the results of their labors. It may be that the tendency to specialization is destined to deprive the sciences of their former hold upon popular interest, and that the study of zoölogy, botany and geology, mineralogy and chemistry will become so technical that each will require the exclusive attention of its votaries for a period of years. It may be that we are to have no more zoölogists such as Agassiz and Baird, no more botanists such as Gray, and that the place which such men filled in the community will be supplied by combinations of a number of specialists, each of whom knows, with more minuteness, limited portions of the subjects grasped bodily by the masters of the last generation. It may be that the use of the word naturalist is to became an anachronism, and that we are all destined to become, generically biologists, and, specifically, morphologists, histologists, embryologists, physiologists, or, it may be, cetologists, chiropterologists, oölogists, carcinologists, ophiologists, helminthologists, actinologists, coleopterists, caricoölogists, mycologists, muscologists, bacteriologists, diatomologists, paleo-botanists, crystallographers, petrologists, and the like.

I can but believe, however, that it is the duty of every scientific scholar, however minute his specialty, to resist in himself, and in the professional circles which surround him, the tendency toward narrowing technicality in thought and sympathy, and above all in the education of non-professional students.

I cannot resist the feeling that American men of science are in a large degree responsible if their fellow-citizens are not fully awake to the claims of scientific endeavor in their midst.

I am not in sympathy with those who feel that their dignity is lowered when their investigations lead toward improvement in the physical condition of mankind, but I feel that the highest function of science is to minister to their mental and moral welfare. Here in the United States, more than in any other country, it is necessary that sound, accurate knowledge and a scientific manner of thought should exist among the people, and the man of science is becoming, more than ever, the natural custodian of the treasured knowledge of the world. To him, above all others, falls the duty of organizing and maintaining the institutions for the diffusion of knowledge, many of which have been spoken of in these addresses—the schools, the museums, the expositions, the societies, the periodicals. To him, more than to any other American, should be made familiar the words of President Washington in his farewell address to the American people:

"PROMOTE, THEN, AS AN OBJECT OF PRIMARY IMPORTANCE, INSTITUTIONS FOR THE GENERAL DIFFUSION OF KNOWLEDGE. IN PROPORTION AS THE STRUCTURE OF A GOVERNMENT GIVES FORCE TO PUBLIC OPINIONS IT SHOULD BE ENLIGHTENED.



WILLIAM STIMPSON.

SOME AMERICAN CONCHOLOGISTS.*

By William H. Dall.

I had selected another theme as the subject of my address on this occasion. But the press of engagements which had to be met prevented the completion of the work required by my first choice, and in looking about for a substitute which would require less original research I remembered that we have not anywhere an epitome of the biography of those naturalists who began in this country the study of the mollusca and who may be truly said to be the pioneers of American conchology.

There was the more propriety in the selection of this topic at the present time since in the year 1887 came the seventieth anniversary of the publication in the United States of the first paper on the American shells, by an American, which ever appeared. We can regard it as forming the extreme limit which might have been attained by a single life, mature enough in 1817 to have appreciated in some measure the dawn of conchological investigation in America. The only naturalist whose life nearly coincided with this period, the late Dr. Isaac Lea, passed over to the majority about a year ago, and, as it happens, his attention was not called to what the French call "the beautiful Science" until 1825.

The contributions of American investigators to the sum of our knowledge of the mollusca have been numerous and important. Many American publications are among the classics of this branch of science.†

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*Annual presidential address, delivered at the Eighth Anniversary Meeting of the Biological Society, January 28, 1888, in the lecture-room of Columbian University.

† Consult Binney (W. G.): Bibliography of North-American Conchology, previous to the year 1860, prepared for the Smithsonian Institution,

But it is not to their publications that I desire to direct your attention, nor to the reputation, due to their labors, acquired for the United States among foreign investigators. It is to the men themselves, the circumstances of their lives, their struggles in an inappreciative age, their unwearied and self-sacrificing devotion to the study of nature.

Of course, in an address of this sort, there is only time for the briefest mention of many facts of interest and value to the biographer; and it would be quite impossible to do even as much as this for all those who have a right to appear on a complete record. So I have confined my attention to some of those who may fairly be considered as pioneers, reserving for another occasion those still active, and many other worthy names.

Following the example of Coues and Goode in their classification of the students of vertebrate zoölogy, I may divide the study of mollusca in this country into three periods, although these are connected by many intermediate links. The infancy of the science, with a Linnæan classification, has no representation in American conchological literature, which sprang, full-grown, like Minerva from the head of Jove, from the Lamarckian school

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Part i. Washington, Smithsonian Institution, March 1863; Part ii, June, 1864, 8vo, viii, 650, and iv, 298 pp. Also TRYON (G. W.): A Sketch of the History of Conchology in the United States (Am. Journ. Science, xxxiii, March 1862, pp. 13–32), and List of American Writers on Recent Conchology, with the titles of their memoirs and dates of publication. New York, Baillière, 1861, 8vo, 68 pp.

There are also a number of portraits of the more distinguished Conchologists given in the first and second volumes of the American Journal of Conchology, though these are not always as good as might be wished.

The above-mentioned works, which contain almost no biographical details, and various dictionaries and encyclopedias have been freely consulted for the material used in this address, but a good deal of it has been the result of personal inquiry, letter-writing, and even advertisement in the newspapers for dates and other missing details. To numerous correspondents I take this opportunity of expressing my thanks for data furnished and which would probably in a few years have been irretrievably lost.

of Europe. The first period might fitly bear the name of its inaugurator, Thomas Say. It is characterized by a rapid advance in the determination of the fauna, the classification of the species, and the exploration of vast areas. It extended from 1817 to 1841.

The second period should bear the name of Dr. A. A. Gould. It was inaugurated by his report on the Invertebrata of Massachusetts, and characterized by the broader scope of investigation, the interest in geographical distribution, the anatomy of the soft parts, and the more precise definition and exact discrimination of specific forms, as exemplified in his writings.

The third period would be appropriately called after Dr. William Stimpson, who eagerly adopted the radical changes in classification rendered necessary by the discoveries of Lovèn, and stood ready to welcome the theory of evolution with all the light it shed in dark places.

Though violently opposed to evolution, the teachings of Agassiz did much to hasten the fruition of the new school of students. For the rational methods of teaching and investigation which he devised or made popular, the present era is greatly in his debt. This period can hardly be said to have been introduced by any epoch-making work, but gradually the old methods were discarded for the new.

The latter were fully exemplified by such works as Morse's "Pulmonifera of Maine" (1864), Stimpson's "Hydrobiinæ" (1865), and a long list of subsequent publications.

Of men belonging to the Sayian period may be mentioned Say, Lesueur, Barnes, Green, Morton, Couthouy, Warren, Anthony, Nuttall, Haldeman, and Conrad.

Rafinesque was *sui generis*, and Lea links this period with the next.

Of the Gouldian period are Gould, Amos Binney, C. B. Adams, Carpenter.

Of the Stimpsonian period I can only refer to Bland, whose place is here rather than with Gould; and lastly, Stimpson himself.

THOMAS SAY.

Thomas Say was born at Philadelphia, of Quaker ancestry, July 27, 1787. His father, as was usual in those days, united to the profession of a physician the duties of an apothecary. Young Say received a very rudimentary education in one of the Quaker schools and at the "Friends' Academy" at Weston, a few miles from Philadelphia. At a later time he studied pharmacy under his father's supervision, and was established in that business with another person whose steady habits it was supposed would ensure success. Among his acquaintance Say's name was always associated with honor and veracity. Conscious of rectitude himself, ingenuous and sincere, he took for granted that others were so, and, as is too often the case, he fell a victim to his trust in others. Having endorsed the business paper of ostensible friends, through their failure he was involved in financial ruin. His heart was not in business, he attended to it with indifference, and, from his school days, was drawn irresistibly toward a study of animated nature. March 21, 1812, he became a member of the Academy of Natural Sciences, then in the process of transformation from a social club to an association of naturalists. The president, William Maclure, seems to have been a warm and intimate friend of Say, and assisted him pecuniarily, for he became the first curator of the embryo museum and lived on its premises for several years, part of the time subsisting on such frugal fare as might be obtained for twelve cents a day! His time was devoted to study and his reputation as a naturalist was already somewhat spread, for he was selected by the publishers to furnish several articles on American Natural History to the American edition of Nicholson's British Encyclopedia, a work which rapidly reached its third edition. In the winter of 1816–17 appeared the second volume, in which the article "Conchology," consisting of fifteen pages and illustrated by four plates, was prepared by Say, and has the honor of being the first paper on American Conchology by an American which appeared in this country. It contained a general statement of the principles of the science as then understood, followed by descriptions of American land and fresh-water shells to the number of thirty-one species. The article was issued separately, with a title page, as "Descriptions of Land and Fresh-water Shells of the United States." The second edition, issued the following year, contained some improvements, and the third edition (1819) had the article considerably enlarged, as it forms twenty pages of the fourth volume of the series.*

The readiness with which Say responded to the requests of others, his liberality in communicating his knowledge to those who sought it, and his agreeable social qualities were the cause of so many interruptions that he was led to devote to study the hours which he should have given to repose, and often worked all night. This injudicious course resulted in serious derangement of the digestive organs, and weakened his constitution. These causes, together with habits of rigid austerity in diet, were probably instrumental in bringing about his premature decease.

In 1818, Say, Ord, Maclure, and Peale made an expedition to the sea islands of Georgia and the country east of Florida, then under Spanish rule. Later, Say was appointed chief zoölogist to the two expeditions to the headwaters of the Mississippi, etc., commanded by Major Long. The same modesty which led him to decline a professorship in an institution of learning on the ground of inadequate scholarship led him to decline the position of historian of Long's expedition after the death of Dr. Baldwin, the first appointee. This modesty led to habits of retirement, and withdrew him from society, except that of his private friends, among whom he was idolized. His domestic virtues were beyond eulogy, and his disposition was so truly amiable, his manners so charming, that no one, having once formed his acquaintance, could cease to esteem him.

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*The first edition is very rare. A copy is said to exist in the library of the U. S. Naval Academy. The second edition occurs in the library of the Boston Athenæum and the Franklin Institute of Philadelphia. The original manuscript is in the archives of the Academy of Natural Sciences of Philadelphia.

These qualities led him to be influenced by those whom he admired, and who possessed a more pushing and self-assertive disposition. It is probable that the great mistake of his life was due to influence thus exerted by his friend and patron, Wm. Maclure.

About the year 1824 the recurrence of one of those waves of sentiment, which, like spots on the sun, appear at intervals, with a certain regularity, to obscure the common sense of the most benevolent and enlightened of mankind, led to the disinterested, though foolish, investment by Robert Owen of large sums in a socialistic enterprise. At the village of New Harmony, in a malarious situation on the Wabash river of Indiana, the sun of righteousness, letters, and science was to rise and illuminate the benighted Western world. Mr. Maclure became convinced of the truth of the gospel according to Owen, and, in 1825, set out for the New Jerusalem, involving in his train his friend Say and several other naturalists. With them went several ladies of intelligence and beauty, one of whom, Lucy Sistare, became the devoted wife of Say, and long survived him.* In a little more than a year the community went to pieces, one founder retiring to Europe, and the other to Mexico, disgusted with the intractability of human nature. It is sufficient to quote a criticism by the son, Robert Dale Owen, himself a member of the community, as given in his autobiography fifty years later:† "I do not believe that any industrial experiment can succeed which proposes equal remuneration to all men, the diligent and the dilatory, the skilled artisan and the common laborer, the genius and the drudge. What may be safely predicted is that a plan which remunerates all alike will, in the present condition of society, ultimately eliminate from a co-operative association the skilled, efficient, and industrious members, leaving an ineffective and sluggish residue, in whose hands the experiment will fail, both socially and pecuniarily."

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*She died in 1886, according to Mr. Schwarz.

†Threading my Way, by Robert Dale Owen. 8vo. New York, Carleton & Co., 1874; p. 290.

But Say had become involved for life. He had married, he had accepted the agency of the property, the duties of which compelled his presence on the spot; he had no other means of support, and therefore resigned himself with his usual philosophy to await the course of events, appropriating all his moments of leisure to his favorite pursuits, and preserving unruffled the serenity of his mind. Mrs. Say prepared drawings and lithographs, and on a little hand-press the early numbers of the "American Conchology" were printed.

The malaria began to influence his health. Had he felt free to follow his medical advice or the affectionate solicitation of his friends, he would have returned to the more genial climate of his native city. But a sense of duty predominated over the claims of affection and the terrors of death, and he remained to become a sacrifice to a fever, which carried him off on the 10th of October, 1834.

I have seen no description of Mr. Say's personal appearance, but his portrait* indicates that his face and expression were in harmony with his amiable character.

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*National Portrait Gallery, vol. iv. Copied in Am. Journ. Conchology, vol. i, 1865. Biography, by Ord, in LeConte's edition of Say's American Entomology, and in Waldie's Select Circular Library, vol. v, 1835, by B. H. Coates, M. D. It seems evident from the hypercritical and patronizing tone of Ord's biography that his old friendship for Say had been severely wrenched, if not broken, by the personal controversies which raged so violently at Philadelphia, and involved nearly all the scientific workers, or those interested in the progress of science, of which Philadelphia was then the American centre. A better biography of Say is greatly needed.

His conchological work was far above the average of its day, and fully abreast of the knowledge of the time.

His monument,* erected in 1846 by Alexander, brother of William Maclure, in the garden of the Maclure mansion at New Harmony, bears the following appropriate lines:

> Votary of Nature, even from a child,
> He sought her presence in the trackless wild.
> To him the shell, the insect, and the flower
> Were bright and cherished emblems of her power;
> In her he saw a spirit all divine,
> And worshipped like a pilgrim at her shrine.

Charles Alexander Lesueur.

Second, in point of time, among those who published in America on American and other mollusks, is Charles Alexander Lesueur,† born at Havre-de-Grace, France, Jan. 1, 1778.

He grew up with a love for natural history so great that in order to accompany the scientific expedition of the "Geographe" under Baudin in the year 1800 he enlisted as a landsman among the crew. Another enthusiast who had, as it were, forced himself upon the expedition was François Péron, who discovered the unusual talents of Lesueur as an artist and succeeded in getting him transferred to the position of zoölogical draughtsman, where those talents could be put to their proper use. Henceforth the two young men were inseparable friends. The commander of the expedition turned out to be most unfit for his position. Besides exhibiting great inhumanity to his subordinates, it is alleged that he was no better than a thief and appropriated to his own emolument the stores of the expedition. He died at last, with many of the others, and finally of the scientific staff only Péron and Lesueur returned to France in 1804. Six years later Péron died in the midst of his labors. Lesueur, inconsolable, was induced to take a voyage to the Antilles and the United States to remove the melancholy which oppressed him. He arrived in the United States in 1816 and settled in Philadelphia the following year, where he taught drawing and pursued his studies, being very cordially received by the resident naturalists. After a residence of nine years in Philadelphia, where he was in a situation most congenial to his tastes and useful to science, he was impelled, through a mistaken sense of duty, to join the settlement of Socialists at New Harmony, Indiana. The presence of Mr. Say rendered the new situation endurable for a time, but with his death in 1834 the delusive expectation that human virtue would increase in the ratio that human individuality was stifled faded completely away, and the position was no longer bearable. He departed for New Orleans and for France, where his tastes and acquirements found their opportunity of fruition at Paris, near the Jardin des Plantes, and afterward at Havre, where a museum was established, of which he was appointed curator in 1845. He was attacked by sudden inflammation of the lungs, which carried him off on the 12th of Dec., 1846, in the 68th year of his age.

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* Recently described by Mr. E. A. Schwarz in Proc. Ent. Soc., Wash., vol. i, No. 2.

† See Memoir, by George Ord, in Silliman's Journal, second series, vol. viii, p. 189, 1849.

Lesueur was a man of unobtrusive and modest manners and social and amicable disposition. Frugal himself, he was generous to others, even in cases where prudence would justify reserve. He suffered from robbery, perpetrated under the guise of friendship, yet with the remnant he had left, and the infirmities of age coming upon him, he shared with others whose necessities were greater than his own.

Lesueur was more of an ichthyologist than a conchologist, but his paper on Firola, in vol. 1 of the Journal of the Academy of Natural Sciences, was the second paper on mollusks published in the United States and the first on exotic mollusks which appeared here.

DANIEL HENRY BARNES.

The Rev. Daniel Henry Barnes, of the Baptist denomination, was born in Canaan, N. Y., April 25, 1785, and was killed by falling from a stage coach between Nassau and Troy, N. Y., October 27, 1828. He graduated at Union College in 1809, and took charge for three years of the classical school there, at a later time. Afterward he was professor of languages in the Baptist Theological Seminary, and in 1824 was associate principal of the New York High School for Boys, an institution he is said to have originated and conducted with great ability. He declined calls to the Presidency of Waterville College, Maine, and the Columbian University, of Washington, D. C. He was a man of high reputation for character and culture, and one of the chief promoters of the New York Lyceum of Natural History, now the New York Academy of Sciences. He assisted Webster in the preparation of his dictionary, and published several early papers on the *Unionidæ* and Chitons, of which he described several forms, while others have been named in his honor by several naturalists.

JACOB GREEN.

Another of the earliest contributors to molluscan literature in America was Dr. Jacob Green, who was born July 26, 1790, at Philadelphia, and died there February 1, 1841. He was the son of Ashbel Green, President of Princeton College in 1812, and grandson of the Revolutionary patriot, the Rev. Jacob Green, who was President of the College of New Jersey in 1757. Our conchologist graduated at the University of Pennsylvania in 1806, was professor of chemistry and natural history at Princeton 1818–22, and then professor of chemistry in the Jefferson Medical College, of Philadelphia, until his death. While his contributions to conchology were not numerous they were of a high order of merit, and on other subjects, such as chemistry, paleontology

(Trilobites), and botany, his work procured him a wide-spread and excellent reputation.

JOHN WARREN.

It may not be amiss to mention here an old Englishman named John Warren, who for many years dealt in shells and curiosities in Boston. About 1857 he was still extant. I have little personal information about him, but remember him as a stout, florid old gentleman, who supplied Miss Sarah Pratt and other Boston amateurs with handsome shells at high prices. In 1834 he published a small quarto edition of Lamarck's genera of shells, illustrated with 17 plates, which he entitled "The Conchologist." He did no original work, but, singularly enough, in Carus and Englemann's Bibliography, he is confounded with Dr. J. C. Warren, the distinguished surgeon of Boston, who published some papers on molluscan anatomy.

SAMUEL GEORGE MORTON.

Among those who have promoted the study of mollusca from the paleontological side, one of the earliest and most distinguished names is that of Samuel George Morton.* Born in Philadelphia Jan. 26, 1799, of Irish ancestry and of a family in which the gifts of education were highly prized and abundantly enjoyed, he early lost his father, and at the age of sixteen entered a counting-room to be prepared for a mercantile career. His desire for study monopolized his leisure, and in 1817 he entered the medical school of the University of Pennsylvania, where he graduated in 1820 with honors, and afterwards pursued his studies at Paris and in Edinburgh. In 1826 he returned to Philadelphia, where he practiced his profession and pursued his scientific studies, and the following year he married Rebecca Pearsall. His career was terminated on the 15th of May, 1851, by an attack of pneumonia, but not until his name, through his scientific work, had become familiar to scholars in both hemispheres. His synopsis of the organic remains in the Cretaceous formation of the United States gave him a high reputation and materially advanced the science. Morton was enthusiastic and energetic, but neither vain nor arrogant. He was drawn into the early controversies which involved the Philadelphian group of naturalists, and appears in them as the especial champion of Say and Conrad. He had a literary turn and strong religious convictions, both of which are perceptible in his scientific publications.

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* See Silliman's Journal, 2d series, vol. xiii, p. 153, March, 1852.

THOMAS NUTTALL.

Although he was especially distinguished in the domain of botany, yet by his shell collections in various parts of America, and somewhat belated studies of this conchological material, it becomes proper to include in this summary, a notice of Thomas Nuttall. Born in Settle, Yorkshire, in 1786, he was in very humble circumstances, and as a journeyman printer had few opportunities for mental development. Yet he was endowed with a strong, clear intellect, the faculty of self-denial, and the passion for study and for the investigation of nature. A hope of improving his position in life and of finding opportunity for study of the natural sciences brought him to the United States in 1808, when only 22 years of age. Through the influence of Barton, the botanist, he was led to take up the study of plants, and a large part of his life was thenceforth devoted to exploration and research. In 1817 he already had been admitted to several scientific societies of high standing. In 1822 he succeeded Peck in charge of the botanic garden at Cambridge, Mass. In 1842 a small estate near Liverpool was left him by a relative, on the condition that he resided upon it at least nine months of every year. He then returned to England, where he died at the age of seventy-three, September 10, 1859. Durand says of him:* "He was a remarkable looking man; his head was very large, bald, and bore marks of a vigorous intellect; his forehead expansive, but his features diminutive, with a small nose, thin lips, and round chin, and with gray eyes under fleshy eyebrows. His height was above the middle, his person stout, with a slight stoop; and his walk peculiar and mincing, resembling that of an Indian. Nuttall was naturally shy and reserved in his manners in general society, but not so with those who knew him well. If silent or perhaps morose in the presence of those for whom he felt a sort of antipathy, yet, when with congenial companions, he was affable and courteous, communicative and agreeable." * * * "I have frequently seen him in social circles when he was the delight of the company, from his cheerful and natural replies to all questions, and his voluntary details on the subject of his travels and adventures." * * * "Nuttall was extremely economical in his habits and careless about his dress. None of his Philadelphia friends, I believe, ever knew where he resided, or in what manner he lived." The profession of science is not a very profitable one, yet, in spite of the few opportunities he had for accumulating, he had succeeded, through the strictest saving, in laying aside enough for his old age, even if he had not inherited the estate of Nut Grove, which was encumbered with annuities and burdened with a heavy income tax.

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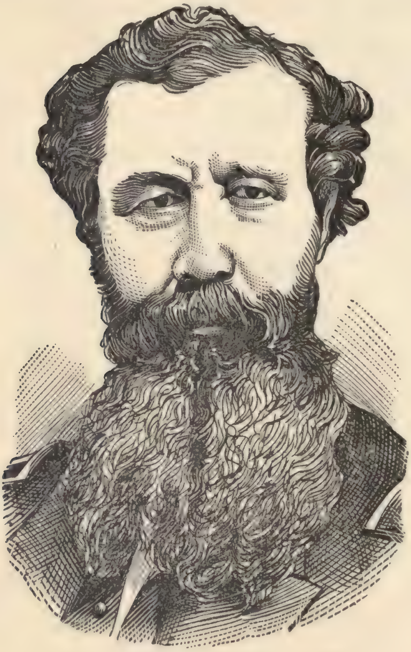
* Biographical Notice, Proc. Am. Philos. Soc., vii, p. 297, 1860.

Nuttall's adventures and privations while exploring among hostile Indians, or during long voyages, were many and exciting, but he declared to his friends that hardships were cheaply purchased if they brought him the opportunity for travel and the contemplation of nature, which he found a source of constant delight.



J. P. COUTHOUY.

JOSEPH PITTY COUTHOUY.

Among the early papers on mollusca in the Journal of the Boston Society of Natural History none are more finished and satisfactory than those by Joseph Pitty Couthouy. Born in Boston January 6, 1808, of French extraction, I learn that he joined the Boston Latin School with the class which entered in 1820. His tastes were for a seafaring life; he shipped on board his father's vessel and rose rapidly in his profession. He married Mary Greenwood Wild, March 9, 1832. He became a member of the Boston Society of Natural History April 6, 1836, and in the reference to his first paper, read October 5, 1836, I find him styled Captain Couthouy. A year later the United States exploring expedition under Wilkes was projected, and, full of enthusiasm, Couthouy came on in person and applied to President Andrew Jackson for a position on the scientific corps. The President said he could not seriously entertain the application as the list of officers was already complete. To which the irrepressible young sailor replied, "Well, General, I'll be hanged if I don't go, if I have to go before the mast!"* This pleased "Old Hickory," who told him, "Go back to Boston and I will see if anything can be done for you." There, a few days after his return, his commission as Conchologist of the Scientific Corps was received. He sailed with the expedition August 18, 1838. After leaving Samoa his health suffered. Wilkes, who was preparing a narrative of the expedition, demanded that Couthouy should turn all his notes and drawings over to his commander. Couthouy refused, as he considered that his subsequent work would be crippled by the absence of notes and drawings already made, and that as a member of the scientific corps he was entitled to retain his papers until the end of the voyage. He was thereupon suspended by Wilkes and ordered home from Honolulu in 1840, "for disobedience of orders."

He had made many valuable drawings and notes, many of which are preserved in the report on the Mollusca and Shells of the expedition. He had numbered his notes with a serial number, and a tin tag, similarly numbered, was attached to the specimen, which was preserved in spirits for future anatomical study and identification. The authorities in Washington had appointed a reverend gentleman who knew nothing of science, with a fat salary, to unpack and take care of the specimens sent home by the expedition. This gentleman, finding that the presence of some lead in the tinfoil tags was whitening the alcohol, carefully removed all the tags and put them in a bottle by themselves without replacing them by any other means of identification. Twenty years ago I saw this bottle of tags on a shelf at the Smithsonian and heard its mournful history. Prominent conchologists resident in the United States were favored, for a consideration, with many rare specimens before any of the expedition naturalists had returned. Some of those contemporary with the events have told me of the prizes secured in this immoral manner, unworthy of a true naturalist, though doubtless the temptation was great.

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* *i. e.*, as a common sailor.

The result of such proceedings may be imagined. Couthouy found that the shells to which many of his notes related could not be identified, and others had disappeared altogether. He worked over the mass that remained until the return of the expedition, when, to crown all his misfortunes, the pay of the naturalists was reduced forty-four per cent., though low enough previously. For Couthouy, who had a wife and two children to support, it was the last straw. He declined to attempt the report, and his papers and collections, after sundry vicissitudes, were put into the hands of Dr. A. A. Gould, who bears willing testimony to the value of Couthouy's work. After this he returned to his profession as a master in the mercantile marine, visiting South America and the Pacific. In 1854 he took command of an expedition to the Bay of Cumana, where he spent three years in the unsuccessful search for the wreck of a Spanish treasure ship, the San Pedro, lost there early in the century. Our next trace of him is shortly after the outbreak of the rebellion. He volunteered in the navy, and, August 26, 1861, was appointed acting volunteer lieutenant. Five days later he was ordered to command the U. S. bark *Kingfisher;* December 31, 1862, to command U. S. S. *Columbia*, which was wrecked, and Couthouy made prisoner. After three months at Salisbury he was exchanged, and, May 29, 1863, ordered to the Mississippi squadron to command the monitor *Osage*, but was transferred to U. S. steamer *Chillicothe.* On the 3d of April, 1864, while off Grand Ecore, Louisiana, on the turret of his vessel, he was shot from an ambush on the shore, and died the following day. The dispatches announcing his death bore testimony to his value as an officer. He was eulogized by Admiral Porter and his fellow officers of the flotilla.

Those who knew Couthouy describe him as active and enthusiastic, with reminders of his French ancestry in his physiognomy and manner; of middle height, dark complexion, and more trim in his dress and refined in his ways than would have been expected from one who had always followed the sea. One friend says of him: "As brave and gallant a soul as ever trod a deck, and a lively and always entertaining companion."

I am informed that he left a son, Joseph P., and two daughters in Boston, and the family is not extinct there. His signature to some documents at the Navy Department is in a handsome flowing hand. He was a good linguist, speaking Spanish, French, Italian, and Portuguese with fluency, and had even mastered several dialects used among the Pacific Islands.

I have not yet come on the track of any published portrait of Couthouy, and none of the biographical dictionaries or cyclopedias refer to him. I have therefore gone into detail a little more fully than I should otherwise have done to preserve from oblivion the memory of a patriotic officer and a good conchologist.

The sketch portrait which accompanies these notes, in default of a better, was derived from an unsatisfactory photograph, the only thing available, taken between 1861 and 1863 and kindly lent to the writer by a surviving relative.

JOHN GOULD ANTHONY.

A naturalist who has left his mark on the classification of our fresh-water shells was John Gould Anthony, who was born in Providence, Rhode Island, May 17, 1804, and died in Cambridge, Mass., Oct. 16, 1877. Mr. Anthony had few educational advantages, leaving school at the age of twelve years, and, going to Cincinnati, engaged in business, where he continued for thirty-five years. In 1863 he was placed in charge of the mollusk collection at the Museum of Comparative Zoölogy in Cambridge by Prof. Louis Agassiz, whom he accompanied to Brazil on the Thayer expedition in 1865. Mr. Anthony was a man of small and delicate frame, with a well-shaped head, whose brilliant dark eyes were a marked feature in his countenance. He suffered in later years from an affection which impaired his sight, and at times prevented him from doing any work. To this cause is due the fact that some of his later work was occasionally wanting in the precision and accuracy which characterized that of an earlier time. He wrote a very beautiful, clear hand, and his labels were as elegant as if engraved on copper. The attractiveness of the Cambridge collection is largely due to his unwearied efforts. A portrait of Mr. Anthony, though not a very good one, was published in the American Journal of Conchology, vol. ii, part 2, 1866. His collection was added to that of the museum at Cambridge.

SAMUEL STEHMAN HALDEMAN.

Samuel Stehman Haldeman was born at Locust Grove, Pennsylvania, Aug. 12, 1812, and died at Chickies on the 10th of September, 1880.

He studied in a classical school at Harrisburg and for two years at Dickinson College, but did not graduate. In 1836 he was called to assist the late H. D. Rogers in the geological survey of New Jersey, and from 1837 to 1842 was engaged in geological work on the State Survey of Pennsylvania. In 1851 he was professor of natural science in the University of Pennsylvania, and from 1869–80 professor of comparative philology in the same institution. He was a member of the National Academy of Sciences. His papers number over two hundred titles, and include such subjects as chess, the natural sciences, and especially philology. He was a distinguished philologist, but to American conchologists his memory will always be grateful, since he was the first to illustrate a work on American mollusks with the beautiful engravings on copper, which were the product of Lawson's burin. These illustrations, though issued as early as 1840, are as fine as anything which can be found in the literature to the present day. Haldeman was short and thickset, with a very peculiar voice, piercing dark eyes, and a pleasant and unaffected manner. He was in easy circumstances, and the freedom which this gave him resulted in a wide and somewhat desultory range of study, and heightened some personal peculiarities of mind.

Timothy Abbott Conrad.

Distinguished among conchologists and paleontologists alike was Timothy Abbott Conrad, born in New Jersey in 1803, who died at Trenton Aug. 9, 1877. Information in regard to him I have found rather difficult to obtain, but it would seem that he was always interested in the natural sciences, especially geology and paleontology, and in 1837 was appointed one of the geologists to the State of New York, and prepared the report for that year. He was paleontologist to the survey in 1838–41. He prepared paleontological reports on the collections of the U. S. exploring expedition under Wilkes, of Lynch's U. S. expedition to the Dead Sea, the Mexican boundary survey, and some of the Pacific Railway explorations. He never married, and during the latter part of his life lived on a small property near Trenton, coming into Philadelphia frequently to pursue his work at the Academy. He was of spare proportions, rather shy and reserved, wrote an abominable hand, and was very careless about his letters, which were largely on scraps of paper without date or location. He drew many of his own plates on stone, and his peculiar style of illustration is very recognizable. Though his contributions to science were multitudinous and long continued, his native carelessness, brief diagnoses, and errors of date and citation gave his work among the more conservative conchologists a reputation perhaps less than its deserts. His defects were chiefly constitutional, rather than wilful; he had an acute and observant eye, and an excellent, if sometimes hasty, judgment on matters of geology and classification. When we consider his work with that of the naturalists of the French "New School" of the present day, there seems in comparison little to complain of in Conrad's methods. Early in life he undertook several journeys to the South especially for collecting purposes, and several naturalists contributed to his expenses with the view of receiving series of the fossils. An unfortunate controversy arose from the conflicting claims to the right and priority of description of many of these species, to which Conrad's extreme carelessness no doubt in a large part contributed. At all events the conflict raged with great violence for several years, and burdened the literature with many synonyms. The matter was still further complicated by the fact that some of his friends, among whom Morton and Say have been mentioned, to preserve, as they supposed, Conrad's rights, wrote and published certain descriptions from his material during his absence and without his knowledge, of which he was obliged, for their sake, to assume the responsibility on his return. To this day the dates of publication of the various parts of his "Tertiary Fossils" are unknown to the public, and were not remembered by the author within a range of several years. Conrad dabbled in literature, and printed a little volume of poems for distribution among his friends. I have heard that all his invaluable documents and manuscripts were sold or destroyed as waste paper shortly after his death through the ignorance of his heirs.

CONSTANTINE SAMUEL RAFINESQUE-SCHMALTZ.

One of the most singular figures in the portrait gallery of scientific men, eccentric as many of them have always been considered, is that of Constantine Samuel Rafinesque-Schmaltz. He was born in Galata, a suburb of Constantinople, Oct. 22, 1783, and died at Philadelphia, Sept. 18, 1840, of cancer of the stomach. His father's name was Rafinesque, and he was of French extraction, but during the hostilities between the French and Neapolitans, which arose about the time he settled in Sicily, he added the name of his mother to his own and represented himself as an American. He arrived in the United States when only nineteen years of age (1802), and returned to Europe in 1805, after which, according to his own account, he was engaged in commercial pursuits and scientific studies at Palermo. He travelled furiously, and collected wherever he went. In 1815 he returned to this country, but the vessel which brought him was wrecked on the coast of Connecticut, and his collections and property were lost, leaving him in a state of poverty from which he never was able to emerge. He was, however, received by American naturalists and others as became his acquirements, and, in 1819, was appointed professor of botany and natural history in Transylvania University, Lexington, Kentucky, which remained his headquarters, in spite of many pedestrian journeys, until 1826, when he removed to Philadelphia, where he remained until his death. His multitudinous writings have been reviewed by Gray, Haldeman, and Tryon in the American Journal of Science, and by Amos Binney in his Terrestrial Mollusks of the United States.*

Rafinesque was a marked example of the adage, "Great wit to madness nearly is allied," and the workings of a mind of unusual acumen, brilliancy, and activity were always clouded by a certain incoherency due to his highly excitable and versatile temperament. He possessed talents which, properly regulated, would have carried him to the front rank of scientific workers.

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* See Silliman's Journal, vol. 40, 1st series, p. 221. 1841; also vol. 42, pp. 280-91, 1842, and vol. xxxiii, 2d series, p. 163, March, 1862; and Terr. Moll., 1, pp. 41-54.

In 1836 we find him insisting, in his Flora Telluriana, that new species and new genera are continually produced by deviation from existing forms. Every variety is a deviation which becomes a species as soon as it is fixed sufficiently to constantly reproduce its kind. Many of the genera he suggested are fully recognized to-day, though by his contemporaries regarded as worthless. But from about 1819 a marked deterioration was noticed in his work, which finally became tinged deeply with a sort of monomania. Societies and journals were obliged to refuse his writings, which poured forth in an ever-increasing flood. When he could obtain means he printed for himself, in shabby and miserable form it is true, but still he printed and projected journals and works which died still-born or never saw the light. His madness seems to have culminated in one of his publications where he describes twelve new species of thunder and lightning.

Of his personal appearance we have the following amusing notes from Audubon's journal:

"A long, loose coat of yellow nankeen, on which the inroads of time were plainly visible, stained as it was with the juice of many a plant, hung about him like a sack. A waistcoat of the same, with enormous pockets and buttoned up to the chin, reached below over a pair of tight pantaloons, the lower parts of which were buttoned down to the ankles. The dignity he acquired from the broad and prominent brow which ornamented his countenance was somewhat diminished by the forlorn appearance of his long beard and the mass of lank black hair which fell from his shoulders." After relating the distance he had walked he expressed his regret that his apparel should have suffered, but at the same time he eagerly refused the offer of any clean clothes, and it was with evident reluctance he accepted an invitation for ablution. The surprise of the ladies of Audubon's family was involuntarily manifested in the exchange of glances which spoke volumes. Soon, however, their astonishment was converted into admiration at the ease and enlightenment of his conversation. Plants and animals with which he was unfamiliar aroused in him a sort of delirium or ecstacy. At night Audubon was surprised by an uproar in the naturalist's apartment. On reaching it to ascertain the cause, he found his guest divested of all clothing, rushing about the room engaged in a sanguinary contest with the bats which had entered by the open window. His weapon was the handle of Audubon's favorite violin, which had been demolished in the fray. Without noticing the entrance of his host he continued his extraordinary gyrations until he was so exhausted that he could hardly use his voice to request that Audubon would obtain a specimen for him, as he was convinced they were of a new species.

Notwithstanding this unpromising beginning, Rafinesque remained three weeks in Audubon's family, who became perfectly reconciled to his oddities and found him a most agreeable and intelligent companion. One evening, however, he suddenly disappeared, without a word to anyone, and it was only after some weeks that a letter was received which assured his entertainers of his gratitude and his safety.

In contrast to his carelessness about his personal appearance, the older Silliman speaks of his beautiful and exact chirography, and says that his communications were always in the neatest possible form. Even in his direst poverty he always retained friends and admirers. It is certain that he must have possessed many lovable qualities.

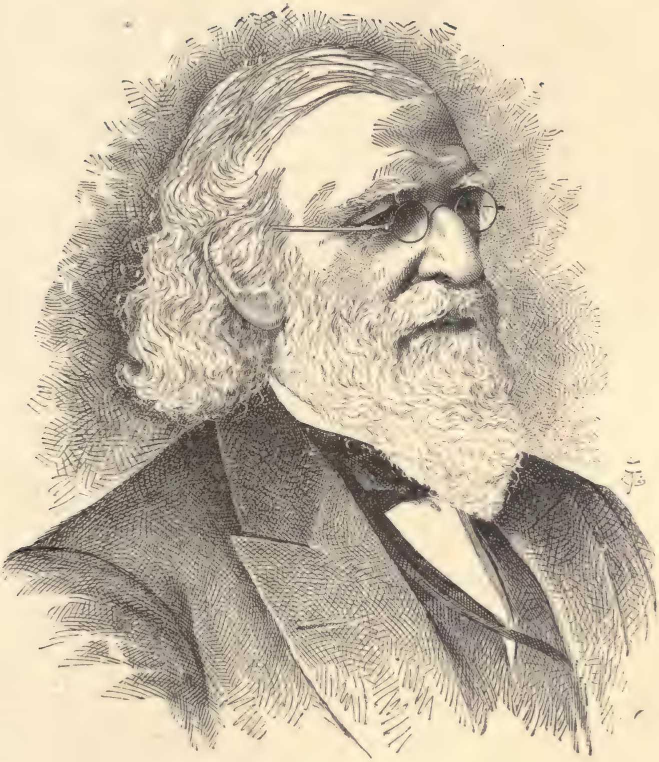
In this connection we may call to mind a friend, Charles A. Poulsen, of Philadelphia, who was devoted to conchology and had a fine collection. Mr. Poulsen translated Rafinesque's "Monograph of the Bivalve shells of the river Ohio" in 1832, and for years his cabinet was resorted to in the vain hope of positively determining some of Rafinesque's ill-defined species. Mr. Poulsen died in Philadelphia in 1866, and I have heard that his collection was dispersed, many specimens being acquired by the late well-known conchologist, C. M. Wheatley, of Phœnixville, Pennsylvania.

ISAAC LEA.

Dr. Isaac Lea, of Philadelphia, whose long and active life gave him among the younger generation the title of the Nestor of American Naturalists, was born in Wilmington, Delaware, March 4, 1792, and died at his home in Philadelphia in his ninety-fifth year, Dec. 8, 1886. His ancestors came from Gloucestershire, England, accompanying William Penn on his second visit. His taste for natural history manifested itself at an early age, and was fostered by his mother, who was fond of botany, and by his association with Vanuxem, then a youth, who was devoted to mineralogy and geology, then hardly organized as sciences. Their studies were undirected; but, in 1815, they became members of the Academy of Natural Sciences, then about three years old. Though engaged in business, young Lea became an active member of the Academy, and published a mineralogical paper in its journal in 1817. This was followed by a very long series of contributions to mineralogy and conchology, recent and fossil, which have made his name familiar to naturalists all over the world.

He married, in 1821, Miss Frances A. Carey, daughter of Matthew Carey, the well-known economist, and became a member of the publishing house of Carey & Sons, from which he retired in 1851. Mr. Lea's married life was exceptionally long and happy, lasting fifty-two years, and blessed with a daughter and two sons, who still survive. One of these sons is the well-known student of ecclesiastical history, while the other has long stood at the head of American photographic chemists.

In 1825 began those studies of fresh-water and land shells, especially the Unios, with which Dr. Lea's name will always be associated. In 1836 he published his first "Synopsis" of the



Dr. ISAAC LEA.

genus, a thin octavo of fifty-nine pages. The fourth edition of this work appeared in 1870, when it had grown to 214 pages quarto.

Dr. Lea was a member of most American and many foreign scientific societies. He visited Europe and studied his favorite mollusks at all the museums. There he made the acquaintance of Férussac, Brogniart, Gay, Kiener, and other distinguished men, whose names now sound like echoes from a past epoch. Up to 1874 he continued ever busy on the Unionidæ, and the number of new forms, recent and fossil, made known by him amounts to nearly 2,000. Not content with figuring and describing the shells alone, he figured the embryonic forms of thirty-eight species of Unio, and described the soft parts of more than 200. He also investigated physiological questions, such as the sensitiveness of these mollusks to sunlight and the differences due to sex. His observations on the genus Unio form 13 quarto volumes, magnificently illustrated. Dr. Lea was president of the American Association for the Advancement of Science in 1860; he presided over the Academy of Natural Sciences in Philadelphia for several terms, and was given the degree of LL. D. by Harvard College in 1852.

His scientific activity extended over more than sixty years. He was active in affairs and vigorously participated in those controversies in which Say, Conrad, Morton, and others were engaged half a century ago. Of these the echoes only have come down to us, but there is plenty of evidence that the battle was often hot and the victory energetically contested.

Dr. Lea had an intellectual and, in later years, a most venerable presence. He was ever anxious to interest the young in scientific pursuits, and was notably active in charitable and religious enterprises. In his youth he manifested more than ordinary artistic talent, much like his distinguished contemporary, Alvan Clark.

It is impossible to do justice to such a life as Dr. Lea's in the proper limits of an address of this sort. It is of the less importance in the present case, because an excellent bibliography of his works, preceded by a biographical sketch and an admirably etched portrait, has been published by the U. S. National Museum,* to whom Dr. Lea bequeathed his invaluable collection of minerals and shells.

AUGUSTUS ADDISON GOULD.

Among those, next to Say, who have beneficially influenced the study of mollusca in this country, and interested young people in that pursuit, no name stands higher than that of Augustus Addison Gould. He was born in New Ipswich, New Hampshire, April 23, 1805, and died of cholera in Boston on the 15th of September, 1866. His father was originally named Nathaniel Gould Duren, but, on account of an inheritance, reversed the order of his surnames. The father was a musician, artist, and engraver, noted for his elegant penmanship, and of a good Chelmsford family; but not in affluent circumstances. From him Dr. Gould probably derived his facility as a delineator of shells. In early life young Gould knew privation, but he persevered in his endeavors for an education, and succeeded in carrying himself through college, graduating at Harvard in 1825, and in medicine in 1830.

He devoted his energies largely to his profession, which he regarded as the work of his life, and in which he soon rose to deserved eminence. But natural science claimed his leisure hours, and to increase them he often robbed himself of sleep. He taught botany and zoölogy at Harvard for two years, was one of the founders and earnest supporters of the Boston Society of Natural History, and original member of the National Academy, and president of the Massachusetts Medical Society in 1865, and until his death. A brother was a member of the well-known firm of Gould & Lincoln, publishers, and through them a number of Dr. Gould's works were republished during his lifetime. It is unnecessary to enumerate his works—the mollusca of the Wilkes exploring expedition, and the magnificent posthumous work on American land shells, edited by Dr. Gould for the executors of Amos Binney, would have given him lasting fame. But the work which was most useful to American science was his classical Report on the Invertebrata of Massachusetts, published by the State in 1841, and adorned with fine copper-plates from his own drawings. This was practically devoted to the mollusks, and served as a manual for New England shells, excellent in every way, and free from unnecessary technicality or pedantic expressions. The speaker well remembers the value this book had for him in his boyish days, and it is said that to it Stimpson owed the impulse which led him, in spite of obstacles, to devote himself to science.

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* Bulletin No. 23, compiled by N. P. Scudder.

Dr. Gould was tall, spare, with dark gray eyes, and hair originally dark, but gray at the time I first knew him. He was the ideal of the "Good Physician," with a winning, sympathetic manner; quiet, and slightly reserved to strangers, but with a living spring of gentle humor for his friends. Full of kindliness, true piety, self-denial, and noble impulses, no one could know him, in the midst of his interesting family, without loving and honoring the man as well as admiring the scientist. The clear, straightforward and exact quality of his work made it easy of comprehension, and there is no knowing how many persons were inspired by it to a study of the animals he described. He was particularly able in his study of the smaller forms of land shells, which he drew with wonderful accuracy and artistic taste. A good portrait of Dr. Gould was published in the Annual of Scientific Discovery for 1861 and afterward reprinted in the

American Journal of Conchology, vol. 1, part 4, 1865.* This picture, though well executed, wants the winning expression which was characteristic of his face.

AMOS BINNEY.

The first to project and illustrate in the highest style of the art a work on the Helicidæ of the United States, doing for the land-shells what Haldeman had attempted for the fresh-water gastropods, was Amos Binney, of Boston, born October 18, 1803, who died at Rome, Italy, February 18, 1847, leaving his work still incomplete. He graduated at Brown University in 1821, and in medicine at Harvard in 1826, but his health proving precarious he devoted himself to commercial pursuits with remarkable success, reserving his leisure for science and art, of which he was passionately fond. He was one of the founders and a liberal giver to the Boston Society of Natural History, which elected him its president from 1843 until his death. He was active in establishing the American Association of Naturalists and Geologists, which has since developed into the American Association for the Advancement of Science.

As a member of the Massachusetts General Court† he was instrumental in securing the organization of the zoölogical and botanical commissions to which we owe the classical Massachusetts Reports by Harris, Emerson, Storer, and Gould.

At his death his work on the Terrestrial Mollusks of the United States was unfinished, but he provided in his will for its completion, a work for which his executors designated his friend and townsman, Dr. Gould, as editor. This work is unsurpassed in elegance of execution by any similar publication to the present day. The premature death by pneumonia of Dr. Binney cut off many promising plans for the promotion of science and art in America. Those interested in land shells, however, do not need to be told that his son, Mr. William G. Binney, has well sustained his father's reputation in the same field. Dr. Binney was above the average height, robust, well formed and refined in appearance. His hair and eyes were very dark, and his expression grave and reserved. This and the somewhat severe tone of his voice was apt to convey to those who did not know him an impression of *hauteur*, which did not correspond to the real feelings of the man. An excellent biographical sketch is given by Dr. Gould in the first volume of the Terrestrial Mollusks, which was published in 1851. Dr. Binney was buried at Mount Auburn, where the monument which commemorates him is one of those to which the stranger's attention is always attracted.

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* A brief notice of Dr. Gould's life appeared in those copies of the second edition of the "Invertebrata" which were distributed by his family. There is a notice by Dr. Jacob Bigelow in the transactions of the Suffolk County Medical Society in 1866.

† So the legislature is styled in that State.

CHARLES BAKER ADAMS.

Charles Baker Adams, one of the most industrious and best known American conchologists, was born in Dorchester, Massachusetts, on the eleventh of January, 1814. Of a family of six children he was the only one spared to his parents. When four years old his father, Mr. Charles J. Adams, removed permanently to Boston, where he engaged in business. At an early age the boy showed great interest in chemistry and natural history, in which he was encouraged by his parents, who gave him the use of a room for a laboratory and furnished the means for procuring chemicals and apparatus. The time usually given to play by most lads of his age was largely occupied by young Adams in experimenting with reagents or studying and arranging the various objects of natural history which he collected in excursions with his father or received from friends. He studied in the Boston schools, at Phillips Academy, Andover, and entered Yale College in October, 1830. In September, 1831, he removed to

Amherst, and joined the sophomore class, graduating in 1834 with the highest honors. Shortly afterward he entered the Theological Seminary at Andover, but in 1836 he left his studies of divinity to join Professor Hitchcock in prosecuting the geological survey of the State of New York. This work being terminated by the illness of Professor Hitchcock he returned to Amherst and busied himself, for several years, partly as a tutor at Amherst and partly by delivering lectures on geology at various educational institutions. In September, 1838, he became professor of chemistry and natural history at Middlebury College, Vermont, and the following February married Mary, daughter of the Rev. Sylvester Holmes, of New Bedford, Mass.

In 1845 he became State Geologist of Vermont, and continued the operations incident to that office for three years. Under his unremitting labors as a popular teacher in the college and his geological work in the field his naturally delicate constitution suffered, and he was obliged to seek a less rigorous climate. He visited the island of Jamaica in the winter of 1843–4, and in 1847 resigned his professorship at Middlebury to accept that of zoölogy and astronomy at Amherst. In the winter of 1848–49 he again visited Jamaica, and in November, 1850, he went to Panama, returning by way of Jamaica the following spring. Anxious to pursue further his investigations on the mollusk-fauna of the West Indian islands, Prof. Adams left for St. Thomas by way of Bermuda in December, 1852, arriving on the 27th, but in his weak condition became a victim of the pernicious malaria of that island, and, though tended with solicitude by his St. Thomas friends, died the 18th of January, 1853. A tablet was placed over his grave by the residents of St. Thomas as a memorial of their esteem and admiration for his character. The Professor's widow, four sons, and a daughter survived him.

Prof. Adams was of middle height, slender and delicate in appearance, with fine expressive eyes and a winning countenance.

In his domestic relations he was gentle and affectionate; in his friendships, faithful and generous. His earnestness and ability as a teacher gave him popularity and success in his college duties, while his private character was above reproach. He was quiet and studious in his habits, but had the true New England genius for hard work; having in his laboratory at the college an old green lounge, where it is said he sought repose in the early morning hours after many a night devoted to original research. Indeed, it is commonly reported among those who knew him that he relinquished to Nature only so much of his time as she imperatively demanded and fairly burned his candle at both ends. Notwithstanding his quiet ways, he was not a man to be imposed upon, and among the college legends, still passed from class to class at Amherst, are several which relate the signal discomfiture of would-be shirkers of their duties, which made him the terror of the lazy men in his classes.

Professor Adams' work was distinguished by care and accuracy, by a philosophical grasp unusual at that day, and which, had he been unhampered by the current theories of the creation and immutability of species, would have given him an even higher rank among naturalists. He monographed the mollusk-fauna of Panama, and did more than any other single naturalist toward making known the riches of the West Indian region. He emphasized the study of the geographical distribution of animals, and as a collector was unparalleled both in enthusiasm and success.

His remarkable collection (probably even now standing third or fourth in the United States in point of interest and value, and its number of contained types) he left under liberal conditions to Amherst College, where it still remains. His publications are among the classics of American conchology, and well bear comparison with many more pretentious works. Like most American naturalists Prof. Adams was never in affluent circumstances, and the success of his labors was largely due to unremitting self-denial.*

PHILIP PEARSALL CARPENTER.

Philip Pearsall Carpenter, who, by his valuable labors on American mollusks and his residence in America, is fairly to be enrolled on the list of American conchologists, was born in Bristol, England, Nov. 4, 1819, and died at Montreal, Canada, May 24, 1877. He belonged to a family whose members have been renowned for their devotion to science, education, liberalism in all good things, and works of benevolence and charity. He described himself as a born teacher, but a naturalist by chance. But his interest in his favorite study developed early. When only twelve years old he had accumulated a large cabinet and mastered the classification of the day. He studied at the University of Edinburgh and at Manchester College, York, which became affiliated with London University, from which he received his degree in 1841. In 1846–58 he labored in the ministry at Warrington, and during this period prepared his classic Memoir on the Mazatlan Shells, and his report to the British Association on the state of our knowledge of the mollusk-fauna of the western coast of America. In December, 1858, he visited the United States and traveled extensively. In the winter of 1859–60 he came to the Smithsonian Institution, where he spent some five months at work upon the shell collections and delivered the lectures on Mollusca which were afterward printed in the Smithsonian Report. In 1860 he returned to England, where he married Miss Minna Meyer, of Hamburg. This union, though entered into somewhat late in life, was most happy. In 1863 he prepared a supplement to his British Association Report of 1856, which has been most useful to students of our west coast shells.

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* His portrait and an appreciative biographical sketch by Thomas Bland, of which I have made unsparing use, may be found in the *American Journal of Conchology*, vol. 1, pp. 191–204, 1865.

In October, 1865, he left England for Montreal, which was thenceforth his home, and where his valuable collection, presented by him to McGill University, is suitably housed in the Peter Redpath Museum of that institution. During the period of his activity in Montreal he devoted himself largely to a monographic study of the *Chitonidæ*, with results of the utmost importance to their proper classification, but of which only a concise abstract has yet been published, though a large mass of MSS. had been prepared at the time of his death.

Dr. Carpenter received the degree of Doctor of Philosophy from the New York State University in 1860. He was a man of slight frame, below the middle height, and of striking personal appearance. He was brimful of enthusiasm not only in his studies, but in all that related to good health, morals, and practical religion. His audacity in confronting and attacking abuses was unparalleled, and, like most reformers, he met with much opposition and made many active opponents. But the rich charity of his nature, his single-minded devotion to what he believed to be right, and his disregard of his personal interests in all that concerned the promotion of reforms, made even the bitterest opponents concede him elements of character of which any man or community might be justly proud.*

Thomas Bland.

Thomas Bland, one of our best known naturalists, was born October 4, 1809, in Newark, Nottinghamshire, England. His father was a physician and his mother related to Shepard, the naturalist. He was educated at the famous Charter-House school, London, and was a classmate of Thackeray. Subsequently he studied and practiced law. He went to Barbados, West Indies, in 1842, and later to Jamaica; visited England in 1850, and in the same year accepted the superintendency of a gold mine at Marmato, New Granada. While a resident of Jamaica, it was visited in 1849 by Prof. C. B. Adams, with whom Mr. Bland cultivated a warm friendship. Stimulated by the enthusiasm of Adams, Bland began those investigations of the land shells for which he afterward became so distinguished. In 1852 he came to New York, which for most of his subsequent life became his home. Here his business lay chiefly in the direction of the affairs of mining companies, with several of which he was connected. He was a man of rather dark complexion, with brilliant dark eyes; somewhat bowed by ill health, induced by his long residence in the tropics, he seemed rather below the middle height. He was of a studious and rather grave demeanor, but notably courteous, and always ready to assist young students or others interested in his favorite pursuit. He avoided controversy, and in spite of his extreme modesty was several times called to posts of honor and responsibility. By those privileged to know him he was held in high esteem, which was not lessened by his bearing under the adversity which unfortunately clouded his later years. Mr. Bland was the author of more than seventy papers treating of the Mollusks, especially of the United States and of the Antilles. His work was not confined to the description of species, but comprised valuable contributions to their anatomy, classification, geographical distribution, and the philosophy of their development. No American conchologist has shown a more philosophic grasp of the subject, and his discussion of the distribution of the land shells of the West Indies, published in 1861, gave him a wide reputation. He several times returned to this subject in later years, and always with marked success. Since 1869 Mr. Bland was associated with Mr. W. G. Binney in several important works on the terrestrial mollusks of North America. Mr. Bland was a fellow of the Geological Society, and for many years an active member of the New York Lyceum of Natural History. He died after an illness of several years' duration in Brooklyn, N. Y., August 20, 1885. A convenient bibliography of his papers was prepared by Mr. Arthur F. Gray in 1884, and his portrait is to be found in the American Journal of Conchology, vol. ii, pt. 4, 1866.

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* An excellent memoir of Dr. P. P. Carpenter, accompanied by a good portrait, was prepared by his brother, the Rev. Russell Lant Carpenter, and published by C. Kegan Paul & Co., London, in 1880.

WILLIAM STIMPSON.

In the case of William Stimpson we have a good instance of how not merely disadvantageous circumstances may be defied but positive opposition conquered by what may be called an innate devotion to the study of nature. He was born in Roxbury, now within the charter limits of Boston, Feb. 14, 1832. His parents were Herbert H. Stimpson, who, I am informed, was of Virginian origin, and Mary Ann Brewster, of a good New England family. Mr. Stimpson dealt in stoves and ranges, in partnership with his brother Frederick, at Congress and Water streets, Boston, for many years. He was a successful business man, though not liberally educated, and introduced certain improvements into cooking ranges, of which one kind was long familiar to Boston housewives under the name of the "Stimpson range." The early education of the son was in the common schools, and in his sixteenth year he seems to have shown unusual mental powers, as we find him entering the upper class of the Boston High School in September, 1847, from which he graduated the following July. Even before this time he had become deeply interested in natural history. A copy of Gould's Invertebrata of Massachusetts having fallen into his hands his attention was directed towards these animals. He presented himself to the author of the work to find out if it were possible for a copy to be had for his very own. Dr. Gould, with his never-varying kindness, gave him an order on the State librarian for one of the books, and the exulting joy with which the boy marched out of the State House with the coveted volume under his arm was never forgotten by him and often related in after years. But Dr. Gould's kindness did not stop here; he brought young Stimpson to the notice of Agassiz, then in the first flush of successful teaching at Cambridge, and introduced him to the Boston Society of Natural History. His relatives were anxious that the boy should go into business; his excursions to the sea-shore and the dredging work which, unaided, he had already begun, were looked on with no favorable eye, and only the urgent representations of some of those who had become interested in the boy and saw in him a capacity for better things, saved him from a fate he detested. As a compromise he was sent out with a civil engineer to learn that profession, but his employer declared he was too fond of hunting for land shells to make a good surveyor, and advised that he be allowed to follow the career which his inclinations so strongly declared for. He was allowed to enter the Latin School in 1848. The following summer he managed by some means to get off on a fishing smack bound for Grand Manan, and devoted his whole energies to the collection and study of the marine animals of that vicinity. Still, in the face of strong opposition, he succeeded in joining the workers at Agassiz' laboratory in October, 1850. Wherever he went his enthusiasm and lovable qualities raised up friends, and through their aid an appointment was secured to him as naturalist to the North Pacific exploring expedition under Ringgold (later commanded by Captain John Rodgers, U. S. N.), which was sent out by the United States in 1852. With a paid appointment in Government service, those who had persistently opposed his ambition began to give way and confess that there might be something in it after all, though doubtless laying greater stress on that "something" for which Stimpson cared least.

He joined the expedition Nov. 23, 1852, and was absent four years, during which he visited Japan, Bering Strait, and many other localities of the greatest interest to the naturalist. No general report on the voyage has yet appeared, and Stimpson's report on the crustacea with its beautiful illustrations still remains in manuscript.

He began to work up his materials at Washington, and for purposes of study visited Europe, dredged on the British coast, and made hosts of friends across the Atlantic.

His preliminary studies of the radiates and crustacea of the expedition ensured his place among the most promising of the young naturalists of the day, and were expressed in elegant Latin. He prepared and published the investigations into marine life made at Grand Manan, and was the leader of an enthusiastic band of students who gathered in the museum of the Smithsonian Institution for work under the influence of Henry and Baird, kept bachelor's hall together under the sobriquet of the Megatherium Club, and instituted the first biological society in Washington under the name of the Potomac-side Naturalists' Club. Most of them subsequently reached distinction in the pursuit of science.

About 1860, Stimpson received the honorary degree of M. D. from the Columbian University. He was afterwards a member of the National Academy of Sciences, instituted while the country was in the midst of its fiercest military struggle. On the twenty-eighth of July, 1864, he married Miss Annie Gordon, of Ilchester, Maryland.

Robert Kennicott, of Illinois, whose name rouses affectionate remembrance in the minds of all who knew him, was Director of the Chicago Academy of Sciences, whose establishment and progress were for the most part due to his enthusiasm, ability, and persistence. He had been a member of the Megatherium Club, and was a devoted friend of Stimpson. He was about to undertake those explorations in Alaska from which he never returned. He knew that his undertaking was arduous, and its outcome uncertain. His child, the Academy, must be provided for, and its fate not left to accident. Stimpson was the man for the post and was selected. The institution was thriving, with a large membership, an excellent collection, and the nucleus of a library. In June, 1866, the building and nearly all its contents became a prey to fire. But the trustees had suitably insured the collection and, with the growing prosperity of the Society, due largely to Stimpson's social tact and attractive personality, the Academy purchased ground, put up a fire-proof building, and rose like a Phœnix with new vigor from the ashes.

Here Stimpson assembled as in a sure harbor the manuscripts, collections, engravings, and drawings of a lifetime.

He had the finest and most complete collection of East American invertebrates which had ever been brought together, with a vast amount of illustrative material from Europe, the Arctic regions, and other parts of the world. Books and specimens which he did not own were freely lent to him by the Smithsonian and by Eastern naturalists, for was he not a scientific missionary, a biological bishop, *in partibus infidelium*, in the land where the almighty dollar reigned supreme? And more important still, the Academy was fire-proof.

A manual of marine invertebrates of the coast from Maine to Georgia was in preparation for the Smithsonian Institution; there was already much manuscript and many beautiful engravings.

All the Smithsonian shell-fish in alcohol were there; Pourtalès sent his unspeakable treasures newly ravished from the depths of ocean. On every hand a wealth of material, a host of indulgent friends and correspondents, a prospect of good work for science, education, patriotism.

On the 8th of October, 1871, a small fire broke out in South Chicago, which was not extinguished. In forty-eight hours the Queen City of the Northwest was practically in ashes.

The temple of religion, the refuge of the sick and destitute, the palace of the millionaire, the shanty of the day-laborer, the sanctuary of trade, the gambler's hell, the hospital, the home, and the grog-shop—withered, crumbled, or evaporated into thin air, before a power stronger than them all.

After this universal destruction, when granite became flour, bricks ran to glass, iron shrunk like wax before the roaring and devouring element, all that was left of Stimpson's lifework, of the building and its treasures of art and nature, was a heap of ashes, the calcined foundations, and the clay pipkin of a mound builder, once rescued from a western tumulus to illustrate the arts of barbarism, and now, in this hour of universal wreck, surviving every product of civilization.

The blow was too heavy. The spirit indeed was valiant, but the body was frail. He had long suffered from weakness of the lungs, with periods of low spirits characteristic of the ailment. After an attempt to work on the Gulf Stream with the Coast Survey in the winter of 1871–2, he returned broken down, and died at Ilchester on the 26th of May, 1872.*

Dr. Stimpson was of middle height, slender, with brown, curly hair, and merry eyes, whose expression was rather heightened than impaired by the glasses he habitually wore. His bearing was that of a scholar, rather retiring, except with friends, when the boyish exuberance of his spirits had full sway. Those who had the privilege of his companionship will carry an abiding memory of his abilities as a naturalist, and his noble and lovable characteristics as a man.

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The number of persons brought under review in the preceding pages (omitting Poulsen and Warren) is eighteen, a number too small to afford many statistical generalizations.

Eight of the men were college bred, ten of them acquired their education in the common schools, or had even fewer early advantages. Two were wealthy by inheritance, two became so by business enterprises, fourteen had a modest or insufficient income, and were obliged to work their way through life; of these five were college bred. Seven were devoted to science among other interests; with eleven science was the mainspring of their lives. The average age attained was sixty years; of those dependent on their own industry about 58 years. Divided according to their absorption in scientific pursuits we find those who devoted all their energies to science averaged 62.27 years, the others 55.7 years of life.

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* See memorial notice by J. W. Foster in *Chicago Tribune* of June 12, 1872. Reported from the proceedings of the Academy.

The only lesson which may be said to be absolutely clear is, that naturalists are born, and not made; that the sacred fire cannot be extinguished by poverty nor lighted from a college taper. That the men whose work is now classical, and whose devotion it is our privilege to honor, owed less to education in any sense than they did to self-denial, steadfastness, energy, a passion for seeking out the truth, and an innate love of nature. These are the qualities which enabled them to gather fruit of the tree of knowledge. Let us see to it that their successors, while profiting by that harvest, fail not in the virtues which made it possible.

DESCRIPTION OF A NEW FOX FROM SOUTHERN CALIFORNIA.

Vulpes macrotis sp. nov.

LONG-EARED FOX.

By Dr. C. Hart Merriam.

(Read Feb. 11, 1888.)

The fox which is the subject of the present communication was killed at Riverside, San Bernardino county, California, November 1, 1885. It differs so strikingly from the other North American foxes that detailed comparison is unnecessary. It is a small animal, the single specimen before me being a little less in size than the Kit Fox (*Vulpes velox*), agreeing in this respect with the California Island Fox (*Urocyon littoralis*), from which latter animal, however, it differs generically. Its most noticeable external peculiarity consists in its large ears, which character alone suffices to distinguish it from its North American congeners.

It is not a little surprising that so large a mammal as a fox, inhabiting so well explored a region as California, should have escaped notice till the present time; and the fact is still more remarkable from the circumstance that the animal here described differs so notably from its nearest relatives. For these reasons, and others derived from a study of the specimen with a view to the known laws of geographical variation, I am led to the belief that it is a Mexican species, finding its northern limit in southern California. The place where the present specimen was killed (Riverside, San Bernardino county) is only a hundred miles from the Mexican boundary.

The following diagnosis is sufficient for purposes of identification:

VULPES MACROTIS sp. nov.

Type No. $\frac{1792}{2324}$, male, young adult, Merriam Collection. RIVERSIDE, CALIFORNIA, NOVEMBER 1, 1885. F. STEPHENS.

EXTERNAL CHARACTERS.—Size, small, equalling or a little less than that of *Vulpes velox;* ears long and broad, relatively much larger than in any other North American fox, and well haired on both sides; muzzle, legs, and tail long and slender, the latter a little longer than the body, and about as slender as in *Urocyon virginianus.* Soles well haired, the plantar tubercles being entirely concealed.

COLOR.—Upper parts grizzled-gray, palest on the head and darkest on the back; terminal fourth of tail nearly black; sides, upper surface of legs, and pectoral band pale fulvous; under parts white mixed with pale ochraceous-buff. In the only specimen at hand the general color is almost as pale as that of *V. velox.* This is due to the fact that the pure white sub-apical zone of each hair is much enlarged, while the black terminal portion tapers rapidly into a much attenuated, awn-shaped point, the result being that the white predominates over the black. The dorsal hairs are short for a fox, and the pale buff of the under fur shows through, thus completing the combination which gives to the back its grizzled-gray appearance. There is no indication of a dorsal stripe on either back or tail. The convex surface of the ear is well covered with short fur which is pale fulvous in color, and mixed with iron gray, except at the base posteriorly where the gray is nearly absent. The margin of the ear is white, as are the long hairs bordering it inside. Between the white border and the grizzled fulvous of the upper surface of the ear there is an indistinct dark line. The base of the ear in front is covered by a dense growth of fur and hair which completely hides the meatus. The lower lip is bordered by a narrow margin of blackish hair, which curves upward around the commissure, and extends forward about one-fourth the length of the upper lip. The chin and throat are entirely white. The whiskers are black, and the hair about their bases is darker than on other parts of the face.

Measurements from the dry Skin.

(All measurements in millimeters).

Total length,	850.
Head and body,	510.
Tail to end of vertebræ,	290.
Tail to end of hairs,	340.
Hind foot,	110.
Height of ear from crown,	68.

Cranial Characters.—The skull is that of a young adult, and probably is not quite full grown; the zygomatic breadth, therefore, is less than it would be in a more aged specimen. Unfortunately, a considerable portion of the occipital region, including both condyles, is broken away; hence the basilar length and several important ratios cannot be taken. The facial part of the skull is much produced and attenuated, the muzzle being relatively longer and more slender than in any other North American fox, and the palatal region correspondingly narrowed. The anterior palatal foramen extends posteriorly to a point opposite the interspace between the canine and first molar. The palatine bones are truncated anteriorly at the post-palatal foramina. The zygomæ arch upward more strongly than usual in the genus, and the audital bullæ are conspicuously larger, deeper, and more rounded, which condition, doubtless, is correlated with the great development of the external ears.

Cranial Measurements.

Basilar length,	*
Occipito-nasal length,	103.
Greatest zygomatic breadth,	58.2
“ breadth across parietals,	42.
“ “ between mastoids,	38.7
Least breadth at interorbital constriction,	19.8
“ “ “ postorbital notch,	20.5
Distance between postorbital processes,	26.3
Palatal length,	55.7
Greatest length of nasals,	40.
Breadth of muzzle at canines,	15.7
“ “ “ midway between canines and root of zygomæ,	14.8
Length of lateral series of teeth (on alveolæ),	51.7
Breadth of palate between canines,	9.7
“ “ “ “ 1st premolars,	9.5
“ “ “ “ 4th premolars,	17.
“ “ “ “ 2d molars,	16.2
Length of mandible,	83.8
Height of coronoid process from angle,	27.
Length of lateral series of teeth (on alveolæ),	57.8
Length of molariform series,	47.5

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* Cannot be ascertained because the condyles are broken off.

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FEBRUARY 11, 1888, TO JANUARY 10, 1890.

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1890

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*Author's separates of the papers especially enumerated were published on the dates given in the parentheses following the author's name.

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The following gentlemen represent for the year, 1890, their respective Societies upon the Joint Commission formed in February, 1888.

Anthropological Society	Dr. Robert Fletcher,
	Washington Mathews,
	Col. Garrick Mallery.
Biological Society	Lester F. Ward,
	C. Hart Merriam,
	R. Rathbun,
Chemical Society	Dr. C. A. Crampton.
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	H. W. Wiley.
Geographic Society	G. G. Hubbard,
	Henry Gannett,
	Everett Hayden.
Philosophical Society	Capt. C. E. Dutton,
	W. C. Winlock,
	Marcus Baker.

PROCEEDINGS.

ONE HUNDRED AND TWENTY-FIRST MEETING,

February 11, 1888.

The President in the chair, and twenty-two members present.

The President announced the death of Dr. Asa Gray, of Cambridge, and of Mr. G. W. Tryon, of Philadelphia.

Dr. C. Hart Merriam presented a communication entitled A NEW FOX FROM CALIFORNIA.* Discussed by Mr. Ward and Mr. Fernow.

Mr. Robert T. Hill read a paper on THE VARIATIONS OF EXOGYRA COSTATA, and a second paper entitled GRYPHEA PITCHERI. Discussed by Dr. Dall.

Prof. C. V. Riley read a communication on THE INSECTIVOROUS HABITS OF THE ENGLISH SPARROW.† Discussed by Mr. Fernow and Mr. Hallock.

A paper by Dr. Theodore Gill—THE CHARACTERS OF THE FAMILY ELACATIDÆ ‡—was read by Dr. T. H. Bean.

ONE HUNDRED AND TWENTY-SECOND MEETING,

February 25, 1888.

Vice-President Ward in the chair, and thirty-four members present.

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* 1888. Merriam, C. Hart. Description of a new Fox from Southern California [*Vulpes macrotis*] <Proc. Biol. Soc. Washn., Vol. iv, pp. 135–138.

† 1889. Riley, C. V. The Insectivorous Habits of the English Sparrow <Bulletin No. 1. Division of Ornithology, U. S. Dept. Agric., pp. 111–133. Issued June 24, 1889.

‡ 1888. Gill, Theodore. The Characteristics of the Elacatids <Proc. U. S. Nat. Mus., Vol. x, 612–614, 1887, pl. xxxix. Issued Sept. 19. 1888.

Mr. F. W. True read a paper on the CHANGES IN THE CATALOGUES OF AMERICAN MAMMALS SINCE 1877.

On motion of Mr. Goode, Mr. True was requested to complete the review for presentation at a future meeting,

Dr. T. H. Bean presented a paper entitled DISTRIBUTION AND SOME CHARACTERS OF OUR SALMONIDÆ.* Discussed by Dr. Vasey, Dr. Merriam, and Messrs. Goode, Cope, and True.

Dr. Cooper Curtice read a paper on SOME EARLY STAGES IN THE LIFE HISTORY OF TÆNIA PECTINATA.† Discussed by a number of members, among them Messrs. True, Cope, Riley, Mason, Goode, Howard, Merriam, and VanDeman.

ONE HUNDRED AND TWENTY-THIRD MEETING,
March 10, 1888.

The President in the chair, and thirty-five members present.

Mr. F. W. True completed his review of the CHANGES IN THE CATALOGUES OF NORTH AMERICAN MAMMALS SINCE 1877. Discussed by Messrs. Goode, Dall, and Merriam.

Dr. Theodore Gill presented a review of THE CLASSIFICATION OF THE COTTOIDEOUS FISHES.‡

Dr. George Vasey read a paper on the FOREIGN TREES AND SHRUBS CULTIVATED IN THE DISTRICT OF COLUMBIA. Discussed by Messrs. True, Vasey, Riley, Stejneger, and VanDeman.

Dr. C. H. Merriam read a DESCRIPTION OF A NEW SPECIES OF AMERICAN SKUNK.

ONE HUNDRED AND TWENTY-FOURTH MEETING,
March 24, 1888.

Vice-President Riley in the chair, and thirty-three members present.

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[*] 1888. American Naturalist, April.

[†] 1888. The Life History of *Tænia pectinata*. Science, Vol. xi, p. 137, March 23, 1888.

[‡] 1889. Proc. U. S. Nat. Mus.

Dr. Cooper Curtice read a paper on TÆNIA FIMBRIATA, A NEW PARASITE OF SHEEP.*

Mr. Charles Hallock presented a paper entitled THE REVERSION OF DOMESTICATED ANIMALS TO A WILD STATE. Discussed by Dr. Merriam, Prof. Cope, Dr. Curtice, and Mr. VanDeman.

ONE HUNDRED AND TWENTY-FIFTH MEETING.

April 7, 1888.

The President in the chair, and thirty-nine members present.

The President announced that the Council recommend that the Society participate in the joint commission of the scientific societies of Washington, and, upon motion, it was resolved that the Society adopt the recommendation of the Council.

The President appointed Messrs. Richard Rathbun and C. Hart Merriam to act with himself as commissioners from the Biological Society.

Captain J. W. Collins read a paper entitled THE WORK OF THE SCHOONER GRAMPUS IN FISH CULTURE.†

Mr. Chas. D. Walcott presented a communication on CAMBRIAN FOSSILS FROM MOUNT STEPHENS, N. W. TERRITORY OF CANADA.‡

Prof. C. V. Riley gave SOME NOTES FROM EMIN PASHA'S TRAVELS IN CENTRAL AFRICA.

Dr. Theobald Smith presented a paper on THE DESTRUCTION OF PATHOGENIC BACTERIA IN THE ANIMAL ORGANISM. Discussed by Dr. Salmon.

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* 1888. *Tænia fimbriata*; The Tape-worm in Sheep. Science, Vol. xi, p. 261, June 1, 1888.

Also, Rept. Bureau Animal Industry, Dept. Agric., 1887–1888, pp. 167–187, plate.

† 1888. Forest and Stream, May 10.

‡ 1888. Am. Journ. Sci., Vol. 36, pp. 161–166.

ONE HUNDRED AND TWENTY-SIXTH MEETING.

April 21, 1888.

The President in the chair, and thirty-five members present.

Mr. F. W. True read a communication on THE AFFINITIES OF THE WHITE WHALE. Discussed by Dr. Merriam and Dr. Dall.

Dr. C. Hart Merriam presented notes on A BAT NEW TO THE UNITED STATES, AND NEW LOCALITIES FOR OTHER NORTH AMERICAN MAMMALS. Discussed by Mr. True.

Prof. C. V. Riley read a paper entitled NOTES ON PLATYPSYLLUS.* Discussed by Mr. Dall.

Dr. Geo. Vasey read Part II of his paper on FOREIGN TREES AND SHRUBS CULTIVATED IN THE DISTRICT OF COLUMBIA. Discussed by Messrs. VanDeman, Ward, and Riley.

ONE HUNDRED AND TWENTY-SEVENTH MEETING,

May 5, 1888.

The President in the chair, and twenty-two members present.

Dr. R. E. C. Stearns read a paper on INSTANCES OF MUTATION IN SPECIFIC DISTRIBUTION AMONG SHELLS. Discussed by Mr. Lucas.

Mr. C. L. Hopkins presented NOTES UPON POLLENATION OF THE NAVEL ORANGES. Discussed by Messrs. Ward, VanDeman, Alwood and Dall.

Dr. C. Hart Merriam read A DESCRIPTION OF A NEW MEADOW MOUSE WITH REMARKS ON THE SUB-GENUS PEDOMYS.† Discussed by Mr. True.

Prof. L. F. Ward presented a communication entitled ON SOME CHARACTERISTICS OF THE FLORA OF THE POTOMAC FORMATION.

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* 1889. Insect Life, Vol. I, p. 300.

† 1888. Merriam, C. Hart. Description of a new Prairie Meadow Mouse (*Arvicola austerus minor*) from Dakota and Minnesota Am. Nat., July, 1888, 598–601, figs. of skull and teeth.

ONE HUNDRED AND TWENTY-EIGHTH MEETING,
May 19, 1888.

The President in the chair, and twenty-two members present.

Mr. F. W. True read some NOTES ON THE HAWAIIAN BAT. Discussed by Mr. Stejneger.

Mr. W. T. Hornaday read a paper on MAN-EATING CROCODILES.

Dr. C. Hart Merriam presented notes on THE NORTH AMERICAN KANGAROO-RATS BELONGING TO THE GENUS DIPODOMYS. Discussed by Mr. True, Prof. Cope, and Prof. Riley.

Mr. F. A. Lucas read a paper on THE AFFINITIES OF CHAMÆA. Discussed by Mr. Stejneger.

ONE HUNDRED AND TWENTY-NINTH MEETING,
June 2, 1888.

The President in the chair, and twenty-four members present.

Mr. F. H. Knowlton read a paper on THE FOSSIL WOOD OF THE YELLOWSTONE NATIONAL PARK. Discussed by Messrs. Ward, Gill, Merriam, and Rathbun.

Mr. W. B. Alwood presented a paper on THE ARTIFICIAL POLLENATION OF WHEAT.

Mr. F. A. Lucas noted SOME ABNORMALITIES IN THE RIBS OF BIRDS.* Discussed by Dr. Gill.

ONE HUNDRED AND THIRTIETH MEETING,
October 20, 1888.

The President in the chair, and twenty-nine members present.

Mr. L. O. Howard exhibited and explained AN APPARATUS FOR THE STUDY OF UNDERGROUND INSECTS AND PLANT ROOTS. Discussed by Dr. Merriam and Mr. Ward.

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* 1888. The Auk, July.

Mr. Lester F. Ward read a paper on THE KING DEVIL.* Discussed by Dr. Merriam and Messrs. Seaman and Ulke.

Mr. Jno. B. Smith read a paper entitled SOME REMARKS ON SEXUAL CHARACTERS IN LACHNOSTERNA.† Discussed by Messrs. Ward, Ulke, and Mann.

Dr. Theodore Gill gave a review of THE FAMILIES OF FISHES. Discussed by Messrs. Ward, Mann, Stejneger, Dall, and Fernow.

ONE HUNDRED AND THIRTY-FIRST MEETING,
November 3, 1888.

The President in the chair, and twenty-one members present.

Mr. F. H. Knowlton presented a paper on FOSSIL WOOD AND LIGNITES OF THE POTOMAC FORMATION.‡

Mr. W. H. Dall read a paper entitled THE MODIFICATIONS OF THE GILL IN UNIVALVE MOLLUSKS.§

Dr. Theodore Gill described THE CHARACTERISTICS OF THE FAMILY SCATOPHAGIDÆ.

Dr. C. Hart Merriam described A NEW SPECIES OF ARVICOLA FROM THE BLACK HILLS OF DAKOTA.||

ONE HUNDRED AND THIRTY-SECOND MEETING.
November 17, 1888.

The President in the chair, and forty persons present.

Mr. Lester F. Ward read a paper on A COMPREHENSIVE

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* 1889. Botanical Gazette, Vol. XIV, pp. 10–17, January.

† 1888. Insect Life, Vol. I, p. 180, December.

‡ 1888. The American Geologist, Vol. III, No. 2. pp. 99–106.

§ 1889. Results incorporated in Report on Blake Gasteropods, Bull. Mus. Comp. Zool., Vol. XVIII.

|| 1888. Merriam, C. Hart. Description of a New Species of Meadow Mouse [*Arvicola longicauda*] from the Black Hills of Dakota <Am. Nat., Oct. 1888, 934–935, figs. of teeth.

TYPE OF FOSSIL CRYPTOGAMIC LIFE FROM THE FORT UNION GROUPS.*

Dr. Cooper Curtice described the SEXUAL DIFFERENCES IN TRICOCEPHALI.

ONE HUNDRED AND THIRTY-THIRD MEETING,

December 1, 1888.

Vice-President Ward in the Chair, and twenty-eight persons present.

Dr. Gill read a paper ON THE RELATIONS OF THE PSYCHROLUTIDÆ.†

Dr. C. Hart Merriam gave a description of A NEW GROUND SQUIRREL FROM CALIFORNIA.‡ Discussed by Prof. Riley and Mr. True.

Mr. F. W. True made some REMARKS ON THE DEER OF CENTRAL AMERICA.§ Discussed by Dr. Merriam.

Prof. C. V. Riley read a paper entitled NOTES ON THE ECONOMY OF THALESSA AND TREMEX.||

Mr. B. E. Fernow discussed THE CAUSES OF CONFIGURATION OF TREES. Discussed by Prof. Riley and Mr. Ward.

ONE HUNDRED AND THIRTY-FOURTH MEETING.

December 15, 1888.

Vice-President Ward in the chair, and twenty-nine persons present.

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* 1888. Abstract in Proc. Am. Ass. Adv. Sci. Vol. XXXVII, pp. 199-201.

† 1888. Proc. U. S. Nat. Mus.

‡ 1888. Merriam C. Hart. Description of a new Spermophile from California. (*Spermophilus beldingi*). <Annals N. Y. Acad. Sci. IV, 317-321, fig. skull. Separates issued December.

§ 1888. Proc. U. S. Nat. Mus., pp. 417-424.

|| 1888. Insect Life, Vol. I, p. 168.

Mr. L. F. Ward read a paper on FORTUITOUS VARIATION AS ILLUSTRATED BY THE GENUS EUPATORIUM, WITH EXHIBITION OF SPECIMENS.* Discussed by Dr. Merriam, Mr. Goode, Prof. Riley, and by Messrs. Stejneger, Vasey, Mann, and Seaman.

Prof. Riley read a NOTE ON A HUMAN PARASITE.

Mr. E. S. Burgess presented a paper on the discovery of ASTER SHORTII NEAR WASHINGTON. Discussed by Dr. Vasey.

ONE HUNDRED AND THIRTY-FIFTH MEETING,

December 29, 1888.

Vice-President Merriam in the chair, and forty-nine members present.

Dr. Theobald Smith read a paper on CONTAGION AND INFECTION FROM A BIOLOGICAL STANDPOINT. Discussed by Dr. Prentiss and Dr. Schaeffer.

Mr. F. A. Lucas presented some NOTES ON THE DISEASES OF MENAGERIE ANIMALS. Discussed by Dr. Merriam, Dr. Salmon, Prof. Atwater, and Messrs. Goode, Hornaday, and True.

Mr. Th. Holm read NOTES ON HYDROCOTYLE AMERICANA.†

ONE HUNDRED AND THIRTY-SIXTH MEETING,

January 12, 1889.

(Ninth Annual Meeting.)

The President in the chair, and forty members present.

The following amendment to the Constitution was proposed by the President:

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* 1889. Abstract in Nature (London), July 25, p. 310.

† 1889. Proc. U. S. Nat. Mus., xi, pp. 455–462, Plates xlvi, xlvii.

In Article X, first line, substitute for "two," the word "three," so that the phrase shall read "*the annual fee* [shall be] *three dollars*."

The annual reports of the Secretary and Treasurer were read and accepted.

The following board of officers was elected for the ensuing year:

President—Lester F. Ward.

Vice-Presidents—Prof. C. V. Riley, Richard Rathbun, Dr. C. Hart Merriam, and Dr. Frank Baker.

Secretaries—F. A. Lucas, Jno. B. Smith.

Treasurer—F. H. Knowlton.

Additional Members of the Council—Dr. T. H. Bean, Dr. R. E. C. Stearns, F. W. True, Dr. Geo. Vasey, C. D. Walcott.

ONE HUNDRED AND THIRTY-SEVENTH MEETING,

January 26, 1889.

The President in the chair, and twenty-seven persons present.

Dr. Cooper Curtice read a paper ON THE SHEEP TICK—MELOPHAGUS OVINUS LINN.* Discussed by Prof. Riley, Mr. Howard, and Dr. Merriam.

Dr. George Vasey gave some notes on NEW SPECIES OF NORTH AMERICAN GRAMINEÆ OF THE LAST TWELVE YEARS. Discussed by Mr. Ward.

Mr. Th. Holm presented a communication entitled CONTRIBUTIONS TO THE MORPHOLOGY OF THE GENUS CAREX. Discussed by Dr. Vasey, and Messrs. Ward, Coville, and Mann.

Dr. C. Hart Merriam called attention to A NEW SPECIES OF PIKA (LAGOMYS).† Discussed by Mr. Knowlton.

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* 1890 (?). In process of publication in Bull.—, Bureau Animal Industry, U. S. Dept. Agric.

† 1889. Merriam, C. Hart. Description of a New Species of Pika (*Lagomys schisticeps*) from the Sierra Nevada Mountains in California. <North American Fauna, No. 2, Oct. 1889, 11–13, pl. viii, figs. 1–6 (skull).

ONE HUNDRED AND THIRTY-EIGHTH MEETING,

February 9, 1889.

The President in the chair, and thirty-seven persons present.

The amendment to the constitution proposed at the annual meeting was brought up for discussion and adopted.

Mr. B. T. Galloway described A DISEASE OF THE SYCAMORE.[*] Discussed by Mr. Crozier and Dr. Vasey.

Dr. Thomas Taylor exhibited and Described A NEW FREEZING MICROTOME.[†] Discussed by Dr. Th. Smith.

Mr. A. A. Crozier discussed THE INFLUENCE OF FOREIGN POLLEN ON FRUIT. Discussed by Prof. Riley, and Messrs. Seaman and Hopkins.

Mr. J. N. Rose read a paper on THE GEOGRAPHICAL DISTRIBUTION OF THE UMBELLIFERÆ.[‡] Discussed by Dr. Merriam.

Dr. C. Hart Merriam gave a description of A NEW AND REMARKABLE VOLE FROM BRITISH COLUMBIA.[§] Discussed by Mr. True.

ONE HUNDRED AND THIRTY-NINTH MEETING,

February 23, 1889.

The President in the chair, and twenty-six persons present.

The President announced that the council had elected Mr. L. O. Howard recording secretary in place of Jno. B. Smith resigned.

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[*] 1888. Reproduced in a paper by Miss E. A. Southworth in Ann. Rept. Dept. Agr.

[†] 1888. Science, Dec. 21.

[‡] 1888. Coulter and Rose. Revision of the N. A. Umbelliferæ. <Herbarium of Wabash College, December.

[§] 1889. Merriam, C. Hart. [Included in] Description of a new Genus (*Phenacomys*), and four new Species of Arvicolinæ. <N. Am. Fauna, No. 2, Oct., 1889, 27–35 pls. ii, iii, iv, vi, and vii.

Mr. E. M. Hasbrouck gave a communication entitled A NEW SPECIES OF MARYLAND YELLOW-THROAT. Discussed by Dr. Merriam.

Mr. M. B. Waite read two short papers under the titles NOTES ON MELAMPSORA HYDRANGEÆ AND NOTES ON THE SEED-VESSELS OF THE LOP-REED, PHRYNE LEPTOSTACHYA. Discussed by Mr. Ward.

Mr. C. D. Walcott gave a note ON THE GENUS OLENOIDES OF MEEK.* Discussed by Mr. Ward.

Mr. L. Stejneger presented some NOTES ON PALLAS' CORMORANT.† Discussed by Mr. Lucas.

Mr. F. V. Coville read a paper entitled THE FRUIT OF STIPA SPARTEA. Discussed by Dr. Curtice and Mr. Waite.

Dr. Merriam exhibited specimens of a new species of Ground Hog or Marmot of the genus *Arctomys*.‡

ONE HUNDRED AND FORTIETH MEETING,

March 9, 1889.

The President in the chair, and thirty-four persons present.

Prof. W. B. Barrows read a paper on DANGEROUS SEED-PLANTING BY THE CROW. Discussed by Drs. Merriam and Vasey, and Messrs. Seaman, True, Ward, and Howard.

Dr. C. Hart Merriam described A NEW SPECIES OF GROUND SQUIRREL FROM WESTERN ARIZONA.§

Mr. C. D. Walcott presented a communication entitled THE GENUS OLENELLUS OF HALL.||

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* 1888. Proc. U. S. Nat. Mus., p. 442.

† 1889. Proc. U. S. Nat. Mus.

‡ 1889. Merriam, C. Hart. Description of a New Marmot [*Arctomys dacota*] from the Black Hills of Dakota. <N. Am. Fauna, No. 2, Oct. 1889, 7–9, pl. viii, figs 7 and 8.

§ 1889. Merriam, C. Hart. Description of a new Species of Ground Squirrel [*Tamias leucurus*] from the arid lands of the Southwest. <N. Am. Fauna, No. 2, Oct., 1889, 19–21.

|| To be published in 10th Ann. Rept. U. S. Geol. Surv.

ONE HUNDRED AND FORTY-FIRST MEETING,

March 23, 1889.

The President in the chair, and thirty-four persons present.

Mr. W. H. Seaman read a paper on OUR PRESENT KNOWLEDGE OF THE ROTIFERA. Discussed by Mr. Knowlton.

Mr. C. L. Hopkins presented A POINT OF DEFINITION relative to the use of the terms *hybrid* and *cross*.

Mr. W. H. Dall described THE REPRODUCTIVE ORGANS IN CERTAIN FORMS OF GASTEROPODA.*

ONE HUNDRED AND FORTY-SECOND MEETING,

April 6, 1889.

The President in the chair, and fifteen members present.

On account of the small attendance and the absence of paper-readers (due to very inclement weather), the program was postponed until the following meeting.

ONE HUNDRED AND FORTY-THIRD MEETING,

April 20, 1889.

The President in the chair, and twenty-one members present.

The death of Dr. J. H. Kidder, an active member of the Society was announced by the President.

Mr. J. F. James presented a paper entitled THE EFFECT OF RAIN UPON EARTH-WORMS.† Discussed by Drs. Merriam and W. H. Fox, and Messrs. True and Ward.

Mr. F. W. True read a paper on the OCCURRENCE OF SOWERBY'S WHALE ON THE COAST OF NEW JERSEY. Discussed by Drs. Merriam and Curtice.

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*1889. Incorporated in Report on Blake Gasteropoda, Bull. Mus. Comp. Zool., Cambridge, Mass., Vol. XVIII.

† 1889. Am. Nat., August. Issued Dec. 15, 1889.

Dr. C. Hart Merriam described A NEW GENUS AND TWO NEW SPECIES OF LEMNING MOUSE OR VOLE FROM BRITISH AMERICA.* (*Phenacomys celatus and P. latimanus*). Discussed by Mr. True.

Mr. Th. Holm spoke on THE GERMINATION OF SARRACENIA, RHEUM, PELTANDRA, HEMEROCALLIS, AND CYPERUS. Discussed by Mr. Ward.

ONE HUNDRED AND FORTY-FOURTH MEETING,

May 4, 1889.

The President in the chair, and twenty-eight members present.

Mr. W. T. Hornaday exhibited and discussed a living specimen of THE BLACK-FOOTED FERRET (*Putorius nigripes*. Discussed by Dr. Merriam.

Mr. B. E. Fernow read a paper on ANNUAL RING-GROWTH IN TREES. Discussed by Dr. Vasey, and Messrs. Vandeman, Ward, and True.

Dr. Theobald Smith presented a communication on PARASITIC PROTOZOA (*Coccidia*) IN THE RENAL EPITHELIUM OF THE MOUSE.† Discussed by Drs. Baker and Curtice.

Mr. H. E. VanDeman described THE TROPICAL FRUITS OF THE LAKE WORTH REGION.

ONE HUNDRED AND FORTY-FIFTH MEETING,

May 18, 1889.

The President in the chair, and thirty-one members present.

Dr. C. Hart Merriam described TWO NEW SPECIES OF SPERMOPHILE FROM THE DESERTS BORDERING THE LOWER

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* 1889. Merriam, C. Hart. Description of *Phenacomys celatus*. N. Am. Fauna, No. 2, Oct. 1889, 33–34.

† 1889. Journal. Comp. Med. and Surg. July.

COLORADO RIVER IN CALIFORNIA AND ARIZONA. (*Spermophilus mohavensis* and *S. neglectus*).* Discussed by Mr. True.

Dr. Cooper Curtice presented a paper entitled HOW ENTOZOA CAUSE DISEASE. Discussed by Mr. Seaman, and Dr. Th. Smith.

Mr. F. W. True exhibited and discussed A SKULL OF A FEMALE NARWHAL WITH TWO WELL-DEVELOPED TUSKS. Discussed by Drs. Curtice, Merriam, Gill, and Messrs. Ward, True, and Murdoch.

Mr. L. O. Howard presented NOTES ON SPIDER BITES.† Discussed by Drs. Merriam, Fletcher, Smith, Marx, and Fox.

ONE HUNDRED AND FORTY-SIXTH MEETING,

June 1, 1889.

The President in the chair, and thirty members present.

Dr. C. Hart Merriam presented A REVISION OF THE GRASSHOPPER MICE AND POCKET MICE WITH DESCRIPTIONS OF NEW SPECIES.‡ Discussed by Mr. True.

Mr. C. D. Walcott read a paper entitled DESCRIPTIONS OF NEW GENERA AND SPECIES OF LOWER CAMBRIAN FOSSILS.§

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* 1889. Merriam, C. Hart. Description of a new Spermophile from Southern California [*Spermophilus mohavensis*]. <N. Am. Fauna, No. 2, Oct. 1889, 15–16.

1889. Merriam, C. Hart. Description of a new Spermophile from Northwestern Arizona [*Spermophilus neglectus*]. <N. Am. Fauna, No. 2, Oct. 1889, 17.

† 1888. Insect Life, Vol. I, p. 347.

‡ 1889. Merriam C. Hart. Descriptions of two new species and one new subspecies of Grasshopper Mouse, with a diagnosis of the genus Onychomys, and a synopsis of the species and subspecies. <N. Am. Fauna, No. 2, Oct. 1889, 1–5, pl. 1 and figs. in text.

1889. Merriam C. Hart. Preliminary Revision of the N. A. Pocket Mice (Genera *Perognathus et Cricetodipus* Auct.) with descriptions of new species and subspecies and a key to the known forms. <N. Am. Fauna, No. 1, Oct. 1889, 1–29, pl. I–IV.

§ 1889. Proc. U. S. Nat. Mus. Vol. 12, pp. 34–46.

Mr. J. F. James read a paper on The Floras of Southern Ohio and Eastern Maryland. Discussed by Messrs. Dall, Gill, Fernow, Merriam, and Ward.

Mr. V. A. Moore presented some Notes on the Morphology of Podophyllum Peltatum.

One Hundred and Forty-seventh Meeting,

October 19, 1889.

The President in the chair, and twenty-four members present.

Dr. C. Hart Merriam described A New Spermophile from the Painted Desert, Arizona.

Mr. Th. Holm presented a paper on the Ancestors of Liriodendron tulipiferæ. Discussed by Dr. Vasey, and Messrs. C. D. White, and Ward.

Dr. Theodore Gill spoke On the Dactylopteroidea.

One Hundred and Forty-eighth Meeting,

November 2, 1889.

The President in the chair, and twenty-seven members present.

Prof. C. V. Riley read a paper on The Remarkable Increase of Vedalia cardinalis in California.

Dr. W. H. Dall read Notes on the Genus Gemma, Deshayes.

Dr. George Marx read a paper entitled A New Spider and its Influence on Classification.

Dr. C. Hart Merriam presented a communication entitled Remarks on the Spotted Skunks (Genus Spilogale) with Descriptions of New Species. Discussed by Mr. True and Dr. Gill.

ONE HUNDRED AND FORTY-NINTH MEETING,

November 16, 1889.

(Postponed Ninth Anniversary Meeting.)

The ninth anniversary meeting (postponed from its regular date on account of Mr. Dall's illness) was held in the law lecture hall of Columbian University, Nov. 16, 1889. A large audience of members and guests was present.

The former President, Mr. W. H. Dall, delivered an address entitled DEEP SEA MOLLUSKS AND THE CONDITIONS UNDER WHICH THEY LIVE.*

ONE HUNDRED AND FIFTIETH MEETING,

November 30, 1889.

The President in the chair, and thirty-eight members present.

Dr. D. E. Salmon read a paper entitled GENERAL REMARKS ON TEXAS FEVER.†

Dr. Theobald Smith followed with a paper on THE MICRO-ORGANISMS OF TEXAS FEVER.‡ The discussion upon both papers was participated in by Prof. Riley, and Drs. Curtice, Salmon, and Smith.

Mr. C. D. Walcott described A NEW GENUS AND SPECIES OF BRACHIOPOD FROM THE TRENTON LIMESTONE.§

ONE HUNDRED AND FIFTY-FIRST MEETING,

December 14, 1889.

The President in the chair, and one hundred and four persons present.

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* Published in this volume. See pp. 1-22.

† 1890. Report Proceedings Public Health Assoc. for 1889 (In press).

‡ 1889. Medical News, Nov. 4.

§ 1889. Proc. U. S. Nat. Mus., Vol. 12.

Dr. C. Hart Merriam delivered an address on the GENERAL RESULTS OF A BIOLOGICAL SURVEY OF THE SAN FRANCISCO MOUNTAIN REGIONS. Discussed by Messrs. Walcott, Diller, VanDeman, and Ward.

ONE HUNDRED AND FIFTY-SECOND MEETING.

December 28, 1889.

Vice-President Merriam in the chair, and nineteen members present.

Dr. A. F. A. King read a paper on THE FLIGHT OF YOUNG BIRDS. Discussed by Messrs. VanDeman, Merriam, Simpson, and Wood.

Mr. M. B. Waite spoke ON A METHOD BY WHICH THE SEEDS OF PILEA PUMILA ARE EJECTED.

Dr. C. Hart Merriam described A NEW RED-BACKED MOUSE FROM COLORADO.

Mr. Th. Holm presented a paper on the GENERIC CHARACTERS OF THE GRAMMINEÆ AND CYPERACEÆ. Discussed by Messrs. Waite, Merriam, Howard, and Seaman.

ONE HUNDRED AND FIFTY-THIRD MEETING,

January 10, 1890.

(Tenth Annual Meeting).

The President occupied the chair, and nineteen members were present.

The annual reports of the Secretary and Treasurer were read and accepted.

The following board of officers was elected for the ensuing year:

President.—Lester F. Ward.

Vice-Presidents.—Prof. C. V. Riley, Dr. C. Hart Merriam, Richard Rathbun, and Dr. Frank Baker.

Secretaries.—F. A. Lucas, and L. O. Howard.

Treasurer.—F. H. Knowlton.

Additional Members of the Council.—C. D. Walcott, F. W. True, Dr. T. H. Bean, Dr. Geo. Vasey, and Dr. R. E. C. Stearns.

ONE HUNDRED AND FIFTY-FOURTH MEETING.

January 24, 1890.

(Tenth Anniversary Meeting.)

The tenth anniversary meeting was held in the law lecture room of Columbian University, January 24, 1890; one hundred and three members and guests present.

The President, Lester F. Ward, delivered his annual address on the subject THE COURSE OF BIOLOGIC EVOLUTION.*

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* Published in this volume. See p. 23.

DEEP SEA MOLLUSKS AND THE CONDITIONS UNDER WHICH THEY EXIST.*

BY WILLIAM HEALEY DALL.

I propose on the present occasion to lay before you a statement of the conditions which characterize the life of Mollusks in the Deep Sea, so far as they are known to us, and to discuss briefly the effect of these conditions upon the animals subjected to them; the contrast which their life presents to that of shallow-water mollusks ; the peculiarities preserved or the modifications induced by the special environment ; together with some notes on interesting or remarkable forms discovered in deep water.

Once for all, it must be understood that exploration of the deep sea fauna has only begun ; that the area swept by the trawl and dredge compared with that which remains unknown, is almost infinitesimal ; and, of the material secured by dredging, a large portion is fragmentary and imperfect. In short what we know about the deep-sea mollusks can only be regarded as a foretaste of that knowledge which future years may be expected to supply.

In an address of this sort bibliographical references would be out of place. I will only say that the literature of the subject is almost wholly confined to the publications of the last twenty years, and consists in large part of the reports by various specialists on such voyages as those of the British vessels, *Lightning*, *Porcupine*, *Valorous* and *Challenger;* the French *Talisman*, and *Travilleur;* the Norwegian, North Atlantic Expedition ; and the explorations of our own Coast Survey, Fish Commission, and Navy on the *Blake*, the *Fish Hawk*, the *Albatross*, and other well known vessels. The most distinguished naturalists of this country and of Europe have added to their reputation by the investigation of the wonderful fauna revealed by these explorations. The most extensive and important single series of Scientific Reports ever published as the result of a single expedition are those which bear the name of the Challenger upon their capacious covers. Next to these come the Reports of the United States Fish Commission, under the leadership of Prof. Baird, and then follows a host of minor documents which it is impossible to enumerate at the present time.

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* Annual Presidential Address delivered at the Ninth Anniversary Meeting of the Biological Society, November 16, 1889, in the law lecture room of the Columbian University.

What I have to present to you this evening is rather a discussion of conditions and principles than an exhibit of particular facts or observations.

In order that their existence may be maintained the abyssal mollusks require oxygen to ærate their circulation, food to eat, and a foothold upon which they may establish themselves. It is necessary that the conditions should be such as will not prevent the development of the eggs by which successive generations are propagated, and that they do permit it may be assumed from the very fact that mollusks in large numbers have been shown beyond all question to exist on the oceanic floor wherever this has been explored.

Formerly when dredging with the usual appliances in small boats, one hundred fathoms (six hundred feet) was considered extremely deep. If one stands at the foot of the great Washington obelisk and looks up, the idea of collecting a satisfactory representation of the insects and plants on the ground at its base by dragging a six foot trawl or dredge by a line let down from the apex of the monument, strikes one as preposterous. Yet the monument is less than one hundred fathoms high. Multiply this height ten or fifteen times and the idea seems, if possible, still more unreasonable, yet it is a fact that successful dredging has been done from a height above the sea-bottom of not less than twenty-five times the height of the Washington monument. Living animals have been secured from a depth equalling the distance from the Capitol to Rock Creek, or from the Washington monument to the mansion at Arlington, that is to say about two and a half miles.

It is therefore evident that in speaking of dredging, we must revise our terms and define them so as to conform more nearly to the new conditions under which such work is done.

The waters immediately adjacent to the shores were long ago divided by Forbes and other pioneers in marine exploration into zones or areas according to the conditions characterizing them ; as, for instance, the Laminarian zone or region of brown kelp, the Coralline zone or region of stony algæ, &c. But for general purposes and to contrast the areas of the whole sea, one with another according to their chief characteristics, we may now divide the entire sea bottom into three regions.

The first is that to which light can penetrate and therefore where marine vegetation can exist. This is the Litoral region and in a general way, modified by especial conditions at particular places, it may be regarded as extending from the actual shore out to the limit of one hundred fathoms. Beyond this it is practically certain that no light reaches the bottom of the sea and no sea weeds grow. Outside of this the borders of the continents slope gradually to the bottom of the ocean, which is found usually at a depth of about 2,500 fathoms.

On the upper parts of these continental slopes the conditions are often very favorable for marine life. Currents of comparatively warm water, like the Gulf Stream, sweep along bringing fresh pure water and supplies of food to the animals along their track. The division between the abysses and the slopes is rather a matter of temperature than of mere depth. But the temperature itself is somewhat dependent on the depth, the influence of the great warm currents rarely extending below seven or eight hundred fathoms and this depth corresponds roughly to a temperature of about forty degrees Fahrenheit. Below this it diminishes as the depth increases, at the rate of about one-tenth of a degree to one hundred fathoms until the freezing point is reached, though there is no reason to suppose that the abyssal water ever actually becomes congealed.

To this cold dark area of the Ocean bottom has been applied the name of the Benthal or Abyssal region.

To the region, chiefly on the continental slopes, between the Litoral and Abyssal regions, I gave some years ago the name of the Archibenthal Region.

These divisions have been recognized by various writers and have had several terms applied to them. Those I have mentioned seem to me as characteristic as any, and in some respects more convenient than any I have heard used.

Let us now consider the conditions under which life exists in the Abyssal and Archibenthal regions. It may be premised that the differences between them are largely of degree and not of kind and do not require that the two regions should be considered separately.

The chief characteristics reside in the composition of the sea water, including its contained gases ; in the dynamic status of the deeps, especially in relation to temperature and pressure ;

in the mechanical qualities of the materials of which the oceanic floor is composed; and, lastly, in the food supply.

As determined by physicists and chemists the water of the deep sea varies in the proportions of mineral salts, carbonic acid and air contained in it very much as does the surface water. In general at the surface the warmer water of the tropics has the more salt and the less nitrogen. When carried by currents to the Polar regions, and cooled, this tropical water sinks to the bottom carrying its excess of salt along with it. The Polar waters are less saline and contain more nitrogen. The proportion of atmospheric air in the water is found strictly related to the temperature, the pressure at great depths being regarded as having no bearing on the question. The amount of oxygen in the sea water diminishes gradually as we descend from the surface until about 350 fathoms is reached, when it ceases to change or at most increases slightly until the bottom is attained.

Carbonic acid, according to Tornœ, does not exist in a free state in sea water, but only in the form of carbonates or to a less degree of bicarbonates. Unless the decomposition of animal matter in some manner sets free the carbonic acid, this conclusion is one which cannot be adopted without question, especially when we consider the great difficulties which are encountered in any attempt to obtain, or when obtained to analyze, abyssal water. The effect of erosion on the shells dredged, from the deeps, even when they contain the living animal, is so strongly marked, the devices for protection against erosion are so recognizable in various species, that the biologist may well call the physicist to a halt, while the latter re-examines his data. It is certain that erosive agencies, of which the effects are indistinguishable from those known to be due to carbonic acid in other instances, are extremely active in the deeps.

In general it seems as if we might safely assume that the composition of abyssal sea water shows no very important differences from that of other sea water and that the animals existing in it are not exposed to any peculiar influences arising from this source alone.

This cannot be said of the physical conditions. Everyone knows how oppressive to the bather is the weight of the sea water at only a few feet below the surface, and how difficult it is to dive, still more to remain on the bottom, if only for a few seconds.

But it is difficult to convey any adequate idea of the pressure at such a depth as 2,000 fathoms, or about two miles below the surface.

Rope made impervious by tarring is said to have become reduced one-third in its diameter by a descent into these depths. Any hollow object not pervious or elastic, is at once crushed. There is no doubt that at some points on the ocean floor the pressure may amount to several tons to the square inch.

If we recall that the average pressure in steam boilers is probably much less than one hundred pounds to the square inch it may help toward an appreciation of the abyssal conditions.

The inevitable conclusion is, therefore, that all the animals living under these conditions must have their tissues so constituted as to permit the free permeation of the water through every part in order that the pressure may be equalized. How this is possible without putting an end to all organic functions is perhaps the greatest mystery of abyssal life. How can a large egg, like those of various deep-sea animals, pass through the stages of segmentation and development, with every molecule of its structure in actual contact with ordinary sea water and every solid particle subjected to a pressure of say a thousand pounds to the square inch?

Such questions are much easier to ask than to answer, in fact no attempt at an answer has, so far as I am aware, ever been offered to biologists.

The looseness of tissue necessary to such a permeation is conspicuous in abyssal animals, whose flabby and gelatinous appearance when they reach the surface is notorious. It is perhaps most noticeable in the fishes, which nevertheless are often armed with formidable teeth. But under the great pressures of the deeps it is quite conceivable that each of these loose and half dissolving muscles may be compressed and reduced to a condition resembling steel wire ; and that the organization thus sustained may be as lithe and sinewy in its native haunts as its shallow water relatives are in theirs.

It is well known how great an influence on the distribution of shallow water species is exerted by the temperature of the water in which they live. No doubt the differences of temperature affect the nervous system, the rate of muscular contraction, and the motions of the cilia by which in mollusks many of the functions of life are aided or wholly carried on.

But it is probable that the influence of temperature is far more effectively exerted upon the development of the ova, and hence upon the propagation of the species, than directly upon the parents. It is probable that most adult mollusks could endure a very wide range of temperature if the individuals were subjected to the changes by extremely slow degrees. But it has been shown that a difference of one or two degrees below a certain point on the thermometric scale, will destroy the embryos of *Ostrea* or prevent their development so that they perish. In this way the spread of the species may be effectually checked, though the adult shellfish may flourish without difficulty in the same region.

In the shallower parts of the Archibenthal Region, a few great currents, like the Gulf Stream, may reach, for a small part of their course, the ocean floor and sweep it clean of sediment and detritus, if not entirely of living beings. Such mechanical effect as is produced must be of a rather steady and uniform nature for considerable periods and in no respect resemble the crushing and grinding which take place on every exposed beach on which the sea rolls up. In fact, regarded as individuals, the mollusks in the path of the Gulf Stream and other great currents, have little or nothing to fear from the mechanical attrition which plays so large a part in the shallows. On the other hand wherever the force of the stream is not sufficient to sweep the bottom clean, the supplies of oxygen and food brought by it to the colonies along its path so far exceed the normal for quiet waters, that the animals thus favored flourish and multiply in a manner never seen in quiet deeps.

The influence of darkness upon the inhabitants of the Abyssal Region has often been expatiated upon. The absence of visual organs or their preternaturally excessive development beyond the normal of the groups to which the individuals belong is evidence enough that the deeps are markedly darker than the shallows. But this evidence proves too much for the claim that the deeps are mathematically dark. Whatever notions may be entertained or conclusions deduced by the physicist from the premises, the presence of large and remarkably developed eyes in many abyssal animals shows that light of some sort exists even on the oceanic floor. It is inconceivable that these organs should be developed without any light and if the experiments and reasoning of the physicist result in the apparent demonstration of absolute darkness in the depths, the facts of nature show that in his premises or his experiments there lurks some vitiating error. It is ridiculous to suppose that the phosphorescence of certain animals in the deep sea fauna is a factor of sufficient importance to bring about the development of enormous and exquisitely constructed eyes in a multitude of deep sea species. A greater or general phosphorescence, such as would amount to a general illumination, has never been claimed by any scientific biologist and, as a theory, requires a mass of proof which seems unlikely to be forthcoming.

In general then we find the physical conditions simpler than those of the shallows and yet much more energetic. The effect of temperature is marked in the distribution of life over cold and warmer areas of sea bottom. The relative importance of the effects of pressure, partial darkness and of the quietness of abyssal waters, our knowledge is yet too imperfect to allow us to precisely estimate. All doubtless have their effect ; some of the effects are more obvious than others, but it is by no means certain that the most obvious are necessarily the most important to the organisms concerned.

The mechanical character of the sea bottom is of greater importance than is generally realized. In a very small proportion of its extent the sea bottom is composed of bare or nearly bare rock. Away from the shores such a bottom is usually situated in the trough of some great current like the Gulf Stream, and then seems to be nearly bare of animal life. In other cases it may be found on the walls of sub-marine cliffs, which for obvious reasons can hardly be explored for marine life with our present appliances.

The rest of the bottom consists of solid matter in different stages of sub-division, from something which may be described as calcareous gravel to an impalpable mud which may or may not be dotted with concretions of manganese, iron or other mineral matter. The gravels are chiefly confined to the archibenthal region, the true deeps are generally carpeted with a viscid layer of the finest possible calcareous mud or clay. The latter formation is meagre in its fauna as clay is when it occurs in shallow water.

Certain forms of mollusk-life flourish in a soft bottom especially the *Nuculidæ* and their allies which are notably abundant in the depths as well as in the muddy shallows of the Litoral Region. Others require some solid substance upon which to perch, a stone, a bit of wood, a spine from some dead Echinoderm, something they must have for themselves and for their eggs which shall raise them above the muddy floor. In regions where such objects are rare or absent on the sea bottom such mollusks are equally rare or wanting. Most ingenious are the shifts made in many cases, as when we find *Lepetella* safely housed in the tubes of dead Annelids or Hydroids, and *Choristes* taking refuge in the empty ovicapsules of rays or sharks. Small hermit crabs take to the tooth-shells (*Dentalium*) or to the tubular Pteropods (*Cuvierina*); or *Amalthea* roosts on an *Echinus* spine and builds for itself a platform as it grows, recalling the arboreal houses of some Oriental savages.

In the Archibenthal Region there is a more or less constant drift of debris from the adjacent shallows which gradually forms banks of considerable magnitude.

The action of erosion and solution for some reason seems less potent here than in either the shallower or the deeper parts of the sea. In the shallower parts the excess of motion, in the deeps the excess of the eroding agent, may account for this. The fact is known to me from the study of many specimens from both regions and is beyond question.

A feature in forming certain of these banks, to which attention has hitherto not been directed, is worthy of mention. This is the habit of certain fishes, which exist in vast numbers, of frequenting certain areas where they eject the broken shells of mollusks, corals, barnacles and other creatures which they have cracked, swallowed and cleansed of their soft tissues by digestion. We have learned from Darwin of the marvelous work of the earthworm in Britain. The ejectamenta of a single fish of moderate size in one day would far exceed the accumulations of many earthworms for much longer time. Now, in examining critically large quantities of material dredged from the bottom I have found that from certain areas almost entirely composed of these ejectamenta. In the interstices some small creatures hide but the tooth marks of the fish were upon nearly every fragment. As for a pint of fragments of a given species, this bottom-stuff would rarely contain half a dozen specimens which had been taken alive by the dredge (most frequently the species did not occur at all living in the material so dredged), it was obviously impossible that the shells could have been captured and afterward voided on the same spot. It seemed more likely from all the facts that these fishes after feeding to repletion repair in large schools to certain areas to enjoy the pleasures of digestion. There would be nothing improbable in the fish of a limited region preferring some special locality for this purpose ; and the result might be the accumulation of a veritable bank, of which nearly the whole had at some time or other passed through the intestine of a fish. At all events, whatever explanation be offered of them, it is certain that such accumulations do occur at certain localities, as shown by the dredgings of the Fish Commission off the eastern coast of the United States.

The last condition remaining to be considered is that of the food supply. It has long since been pointed out that marine vegetation ceases to exist within a limit of six hundred feet below the surface. Whatever light exists in the depths, it is not of a nature to meet the needs of vegetation. Whether any other factor joins with the absence of light to discourage algal growth is yet unknown but not intrinsically improbable. The mollusks which belong to groups known as phytophagous in shallow water, in the deeps appear to live chiefly on foraminifera which they swallow in immense quantities. The results of this diet are evident in the greatly increased caliber of the intestine relatively to the size of the animal, in the diminution of the masticatory organs, teeth and jaws, and in the prolongation of the termination of the intestine as a free tube to a length which will carry the effete matters out of the nuchal commissure, and thus free from their injurious effects the branchial organs, which are usually seated in this space. The quantity of nutriment in the protoplasm of foraminifera is so small that a much larger mass in proportion of these organisms must be swallowed and their remains consequently ejected afterward, than if the food consisted of the tissues of algæ.

But the great mass of abyssal mollusks are members of those groups which in shallow waters are normally carnivorous, and to a great extent prey upon one another. In the deeps however this reciprocal destruction is unnecessary.

Those who have become familiar with surface collection on the sea alone can realize the immense quantity of organisms which exist in the water on or near the surface. These are frequently numerous enough to reduce the water to the consistency of soup, for miles in extent and to a considerable depth. Millions of these creatures are constantly sinking from the region where they naturally belong, either from injury or exhaustion, and thus raining slowly but constantly upon the bottom. This fact is not new and is admitted to be unquestionable by all biologists. Hence in many regions of the sea bottom the resident fauna have, as it were, only to lie still and hold their mouths open.

One of the facts which attracted my attention when I first begun to study deep sea mollusks was the singularly small number which showed signs of having been drilled or attacked by other mollusks. Apart from those showing the marks of fish teeth or the dental machinery of echinoderms, it is extremely rare to find drilled bivalves or univalves such as make up the great mass of the jetsam on every sandy beach. Such cases occur, but the occurrence is always exceptional and the holes which are most often found in abyssal shells are those which are due either to the friction of some hermit crab or to the erosive properties of the secretions of certain annelids which fix their irregular tubes upon the outer surface of the shell. These injuries cannot easily be confounded with the circular drill holes of carnivorous gastropods.

Having handled more deep sea mollusks than any other naturalist now living, and spent, probably, more time over material procured by the dredge from shallow water, than anyone else of my acquaintance, I do not feel that I am presumptious in affirming the remarkable difference which obtains in this respect between the dead material from the Litoral and from the extra-Litoral regions, respectively.

This brings me to a conclusion which I have elsewhere published with less detail. The animals belonging to the mollusca which are found in the Archibenthal and Abyssal regions, especially the latter, do not live in a perpetual state of conflict with one another. A certain amount of contention and destruction doubtless goes on, but on the whole the struggle for existence is against the peculiarities of the environment and not between the individual mollusks of the area concerned. It is an industrial community, feeding, propagating and dying in the persons of its members and not a scene of carnage where the strong preys upon his molluscan brother who may chance to be weaker. Depredations on this community are doubtless committed by deep sea fishes and echini, perhaps by other organisms, but the inroads are not so important as to seriously modify the course of evolution and influence specific characteristics.

Hence the course of evolution and modification, though still complex, is certainly much less so than in the shallower parts of the ocean. For this reason we may hope to penetrate more deeply into its mysteries with deep sea animals than with those less fortunately situated. In this opportunity, to me, lies the chief importance of research into the biology of deep sea mollusks. Nowhere else may we hope to find the action and reaction of the contending forces less obscure, and modification in most cases has not extended so far that we cannot compare the deep sea forms with their shallow-water analogues and draw valuable conclusions.

While we are not yet in a position to formulate conclusions covering all the details of abyssal mollusk-life in certain instances results suggest themselves.

Deep sea mollusks of course did not originate in the depths. They are the descendents of those venturesome or unfortunate individuals who, by circumstances carried beyond their depth, managed to adapt themselves to their new surroundings, survive and propagate. Many species must have been eliminated to begin with. Others more plastic, or more numerous in individuals, survived the shock and have gradually spread over great areas of the oceanic floor. In accordance with these not unreasonable assumptions we should expect to find among the newer comers at least some characters which were assumed under the stress of the struggle for existence in the shallows, and which, through specific inertia, have not become wholly obsolete in the new environment. We should also expect to find a certain proportion of Archibenthal species in any given area, identical with or closely related to the analogous Litoral region forms of the adjacent shores.

In the Abyssal region alone should we expect to find that any considerable proportion of the fauna has lost all its litoral characteristics, assumed characters in keeping with its environment and become disseminated over the ocean bottom throughout a large part of its extent. These expectations in the main are fairly satisfied by the facts as far as the latter are positively ascertained.

With the lesser need of protection from enemies and competitors would necessarily be related a less rigorous elimination of characters which in struggle and competition might prove sources of weakness. The limits of uninjurious variation would be relaxed at the same time and to the same extent. We find as we should expect that the deep sea mollusks are more variable in their ornamentation and other superficial characters than those from shallow water. In some species the balance of characters is fairly well maintained ; in others variation runs riot, and it is impossible to say what amount of it should constitute a basis for specific subdivisions among individuals.

In general deep sea shells present pale or delicately tinted color patterns, are white, or owe their color to the tinting of the epidermis. This may be due directly to the absence of light. Sunlight, when present, seems to have a stimulating effect in developing colors as is shown by the greater brightness of tropical litoral shells whatever their colors. It operates indirectly by promoting the development of color in algæ which are fed upon by phytophagous mollusks, and affect the coloration of the latter directly through the assimilation of the coloring matter of the food, mechanically.

Indirectly, through the influence of protective mimicry, the coloration of shells which frequent beds of seaweed or rocks covered with stony algæ, is often modified in harmony with the environment even when the species is not phytophagous.

In the deeps these influences are wanting and the development of color is necessarily the result either of uneradicated hereditary tendency, or of some physical features of the environment which operate mechanically and are not yet understood.

The colors chiefly effected by deep sea mollusks are pink or reddish straw color, salmon color, and various shades of brown. These are found in the shell and are more or less permanent. The epidermis of deep sea shells is usually pale yellowish, but frequently is of a delicate apple green, such as is seen in many fresh water species; and sometimes of a beautiful rich dark chestnut brown, a color also not rare among land and fresh water species. The most common pattern when any exists is that formed by squarish dark spots which occasionally become fused into bands. Among the Archibenthal species found in depths from 100 to 300 fathoms this pattern of brown squarish spots arranged in spiral series is notable in such forms as *Scaphella junonia*, *Aurinia dubia*, *Halia priamus*, *Conus mazei*, etc. Instances of the green epidermis are afforded by the various species of *Nuculidæ*, *Turcicula* and *Buccinidæ*.

The thick and solid layers of aragonite, of which many shallow water species are chiefly built up, are represented in deep water forms by much thinner layers, while the nacreous layers are, if not more solid in abyssal shells, at least more brilliant and conspicuous, perhaps because less masked by aragonitic deposits. A very large proportion of the deep water shells are pearly and derive their beauty from the brilliance of their nacre.

In the matter of sculpture the mechanical effect of the pressure operates against the development of weight and thickness in benthal shells since the whole must be permeable. It is probable too that the soft and sticky character of the abyssal ooze would put the possessor of an unusually heavy shell at a considerable disadvantage in getting about on the bottom. Any impermeable shelly structure on the ocean floor would have to be strong enough to sustain without crushing a weight hardly less than that borne by the rail under the driving wheel of an ordinary locomotive. It is sufficiently obvious from a mere statement of the case, that none of them can be impermeable.

The heavy knobs or arborescent varices of shallow water Murices are represented in their deep water congeners by extremely thin and delicate spines and slender processes. These are probably all reminiscences of shallow water ancestors, as it is difficult to imagine any cause which in the abysses would lead to a development of such defenses *de novo.*

The sculpture most usual on deep water shells is of a kind which serves to strengthen the structure, much like the ridges which give rigidity to corrugated iron work or the curves used by architects in wrought iron beams. Spiral or longitudinal hollow riblets, a transverse lattice work of elevated laminæ such as are developed for similar reasons on the frail larval shells of many gastropods, a recurvature of the margin of the aperture in forms which in the Litoral region never develop such recurvature ;—these are instances in point.

Beside these there are small props and buttresses developed which serve the same purpose of strengthening the frail structure at its points of least resistance. Such is the garland of little knobs so commonly found in front of the suture in abyssal shells of many and diverse groups.

It is not intended to suggest that the methods above indicated have not been developed also in shallow water forms and for similar reasons. The distinction which I would point out is that in Litoral species, as a rule, these devices are subsidiary to the much simpler course of strengthening the shell by adding to its thickness. In the abyssal forms, for reasons already explained, this mode is not practicable and consequently we have the one without the other.

The operculum is generally horny in abyssal mollusks, frequently disproportionately small, compared with that of congeneric litoral species, and in a remarkably large number of cases is absent altogether.

The genus most abundantly represented of all is *Mangilia*, which is entirely without an operculum, and affords a conspicuous example of the obsolescence of protective devices, originally acquired in shallow water, resulting from long residence in the deeps.

In the Unio and Melania of fresh water streams and the pondsnails of our lakes and ponds, the waters of which from the decay of vegetable matter are overcharged with carbonic acid, we find a dense thin greenish epidermis developed as a protection against erosion. In the deep sea where every portion of the shell must be permeated by the surrounding element to equalize the external pressure, and where carbonic acid exercises its usual malign influence on the limy parts of all organisms, we find a strikingly similar protective epidermis developed in most unexpected places. Thus it comes about that in the *Trochi*, *Pleurotomidæ* and other characteristic abyssal animals we find those puzzling and remarkable counterparts of land and fresh water shells which have astonished every student of the mollusca who has seen them. These deep water species imitate in almost all superficial characters of the shell the biologically wholly different pondsnails and landsnails.

Similar exigencies of the environment have provoked similar mechanical responses in the shelly parts, a result wholly in harmony with the modern postulates in biological science.

As might be expected oi descendents with modification there are greater similarities between the larval shells of benthal species and those of their shallow water relations, than between the parts of the shell, which are of later growth in the same forms. There is one notable difference however. In the deep water forms the nucleus is frequently larger than in their shallow water analogues. It would seem as if the conditions of the depths were such, that, of a small number of large larvæ, more are likely to survive than of a larger number of smaller ones ; or at least that that form of larval growth is more useful to the species.

These details will serve to show the multiplicity of facts to be accounted for and the opportunity for advancing science by a study of abyssal conditions and their effects upon the animals subjected to them. Without claiming any unique importance for the theories advanced in the foregoing remarks it may still be said that the subject is one of the very greatest interest. Perhaps experiments upon shallow water forms, artificially subjected to pressure may at some future time enable us to penetrate more deeply into the mysteries of life in the abysses.

The attempt to prepare a summary of bathymetrical data for the deep sea fauna of any region yet investigated, is most unsatisfactory in its outcome from the paucity of data. Most of the species of any collection are represented by the shells alone, which may have been—as millions are daily—disgorged by fishes, and never have lived at the depth from which they were dredged. We are yet ignorant as to whether the abyssal and archibenthal faunæ shade gradually into one another, as seems most probable; or whether there is any line of depth, coincident with a temperature limit, which really fixes a boundary for the abyssal fauna.

Then, again, the difficulty and time involved in a cast of over one thousand fathoms are so much greater than if it were made in half that depth, that it is impossible to say what proportion of the disparity in population between the Archibenthal and Abyssal areas, which dredgings seem to indicate, is due to the fact that the latter have been far less efficiently explored. The only thing of which I feel confident is that it is yet too early for extensive numerical comparisons or deductions based wholly on statistics. I shall therefore content myself here with a very modest table, which is intended to illustrate the peculiarities of the collection made during the past ten years by the U. S. Steamer *Blake* and recently reported on by me.

It is probable that it is a fair example of abyssal mollusk faunas, but this cannot be claimed with certainty.

The first table shows the general numerical results for the *Blake* collection, assorted among the great systematic groups and the three bathymetric zones or areas. The second table shows the proportion to the whole population of the abyssal region borne by those genera which exceed a single species. The result here shown is that less than thirty-seven per cent. of the genera comprise more than sixty-eight per cent. of the species; and out of these, three families, *Pleurotomidæ*, *Ledidæ*, *Dentaliidæ* furnish nearly twenty-eight per cent. of the species of the abyssal fauna collected by the *Blake*.

TABLE I.

General Numerical Results.

Groups	No. of Genera	No. of Species	Species in the			Species common to		Abyssal Fauna	
			Litoral Area	Archib. Area	Abyssal Area	Two Areas	All Areas	Families.	Genera
Brachiopods .	7	13	8	12	3	8	2	2	3
Pelecypods .	52	170	98	114	31	64	10	15	19
Scaphopods .	2	35	17	28	12	17	5	1	2
Gastropods .	119	491	280	222	83	161	32	29	41
Totals . .	180	709	403	376	129	250	49	47	65

TABLE II.

Genera represented by more than one Species in the Abyssal Area.

Genera.	No. of Species.	Genera.	No. of Species.
Mangilia	17	Fluxina	2
Margarita	5	Liotia	2
Pleurotoma	4	Leptothyra	2
Drillia	3	Cocculina	2
Marginella	3		
Scala	3	Leda	5
Calliostoma	3	Limopsis	3
Triforis	3	Pecten	3
Actæon	3	Abra	2
Utriculus	2	Myonera	2
Fusus	2		
Columbella	2	Dentalium	8
Benthonella	2	Cadulus	4

Total, 24 Genera and 87 Species.

For the naturalist of to-day the most interesting feature of abyssal life is not that it furnishes him with singular and archaic forms, useful in his study of extinct genera ; nor the beauty and rarity of the creatures living under such unusual conditions. The most important characteristic of abyssal life is, that it, and it alone, exhibits a fauna in which reciprocal struggle is nearly eliminated from the factors inducing variation and modification. There is no mimicry or sexual selection, where all is dark. Indeed, if it could be shown that the deeps are absolutely dark, the acknowledged development there, by some animals, of large and supersensitive eyes, might be a proof of the Lamarkian doctrine of development consequent on effort, as opposed to the views of Darwin, that it is solely the result of selection conscious or unconscious and the survival of the fittest.

In the struggle for life of the abyssal animal, he is pitted against the physical character of his environment, and not against his neighbor or the rest of the fauna. Hence we should have, and really do have, the process of evolution less obscured by complications in the abysses than is possible elsewhere. From a study of these animals in the light of their environment, much may be hoped toward the elucidation of great questions in Biology, and naturalists everywhere should strive to promote deep sea dredging as essential to the progress of Science.

THE COURSE OF BIOLOGIC EVOLUTION*.

BY LESTER F. WARD.

That organic forms are the product of evolution is now not only generally accepted by educated people, but is also fairly well understood as a general proposition. But the special nature of the evolutionary process, particularly the *modus operandi* of the laws of development, is only vaguely or crudely comprehended by any but specialists in some branch of biology, and is not clearly understood by all of these. In proof this I recall a lecture by Henry Ward Beecher, delivered in this city within a year of his death, in which he attempted to expound the modern scientific doctrine of evolution, but in which he showed that he had no adequate idea of what is meant by the arborescent, much less by the dichotomous character of the process of organic development, and seemed to suppose that the progress from monad to man had been one continuous ascending series. He mentioned, for example, as among the ancestors of man, a number of animals belonging to the Ungulata, Carnivora, etc., which are known to be entirely off the anthropogenetic line.

Such crude exposition of so important a law as that of evolution can only react against the progress of its acceptance as a scientific truth, and there seems to be great need that the exact nature of this law be worked out, and that all attempts to popularize it be correct and be accompanied by the necessary qualifications and an explanation of important subordinate laws. Only thus can the coarse and repugnant conceptions which seem to be taking possession of the popular mind be removed.

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EXTINCTION OF TRUNK LINES OF DESCENT.

It is especially important that the first great qualifying principle, which I propose to call *the law of the extinction of trunk lines of descent*, be made clear, since it lops off at one stroke, the most serious of all popular misconceptions. I shall assume that the principle of genealogic dichotomy is clear to the minds of all, since it is nothing more than the simple law of tocogonic descent as exemplified in every human genealogy and every family register applied to all life, except that it relates to species instead of individuals.

Sympodial Dichotomy.—But while organic phylogeny is, in a certain sense, arborescent and dichotomous it cannot be directly compared to any ordinary tree nor even to a plant that branches in a strictly forking or dichotomous manner, such as an *Anychia*, for example. It resembles more nearly that form of indeterminate growth which is termed *sympodial*, in which, instead of the two forks being equal and divergent, one of them has to be regarded as the main trunk and the other as a branch, but in which the branch possesses the greater vigor and vitality and virtually becomes the main trunk, the true stem dwindling, and either dying out entirely or continuing as a reduced and degenerate form. There are many plants, such as the common grape-vine, the houseleek, the heliotrope, and the for-get-me-not, that exhibit this sympodial dichotomy.

Types of Structure.—In studying the operation of this law in biology a number of important facts are to be noted. It is first necessary to consider what may be called *types of structure.* These, in our illustration of sympodial dichotomy, represent first, the main trunk, and afterward the successive branches which become virtually the main trunk. Except in parasitism and other anomalous cases, the development along the main trunk is in the nature of an ascending series of forms, in the sense that the structure grows more and more perfect. There is a differentiation of organs and functions and an integration of parts into organisms of higher and higher capacity, but all are constructed upon the same general plan and represent a single and uniform type. This process of perfectionment in the organisms of original type constituting the main trunk proceeds as far as the nature of that type of structure will permit. The branch which is to constitute the new and higher type is ultimately developed out of this original trunk, but there is no fixed time for its appearance. The original type may have reached its maximum development and remained stationary for any length of time, or it may have already begun to decline before this takes place. In fact it may never take place, but such organisms perish and leave us no history. The branch must possess a higher type of structure, otherwise it must dwindle and also disappear. To give it fresh vigor and power to continue the stock it must have acquired, through the known laws of variation and selection, some advantageous character not possessed by the original type, to which its superior vigor is due. It then in turn continues to develop and goes on improving in the same manner as the main trunk did before it, until, like that, it reaches the maximum limit to its capacity for progress, *i. e.*, until nothing more can come from that type of structure. Like its ancestor, too, it then remains stationary for an indefinite period and eventually declines, and either persists in a degenerate form or dies out altogether. A second branch endowed with still higher capacities is developed from the first and this repeats the process, and so on indefinitely, higher and higher types being successively developed, carrying up the system by this process of ascending sympodial dichotomy.

Persistence of unspecialized Types.—It often happens that the highest organisms of the more ancient types become extinct while the lower or less perfect ones persist and are found mingled with organisms of the higher types that are the dominant forms of life at subsequent epochs. This fact has led those who did not understand the law of types, as just stated, into doubts relative to the fact of development, since the certainty that organisms belonging to types that still exist, but of much higher rank, formerly inhabited the globe gave rise to the belief that there has been degeneracy instead of progress. To escape this error it is necessary to understand that progress takes place primarily through the development of new and higher types of structure, embodying successively higher and higher capacity for improvement, and that the archaic forms belonging to lower types, and therefore, as it were, upon a lower plane of life, unable to compete with those of higher type, are repressed and only appear among the latter as humble, and, as regards their own ancestors, really degenerate forms.

We thus have a series of epochs in the earth's history during each of which a different type has predominated, each later type being higher in its capacity for improvement than its predecessor. You are all more or less familiar with the successive reigns of articulates in the Cambrian, mollusks in the Silurian, fishes in the Devonian, reptiles in the Mesozoic, and mammals in the Cenozoic; and you have doubtless frequently heard astonishment expressed at the great perfection to which the articulated type attained in the Trilobite, the molluscan type in the Ammonite, the piscine type in the Ganoid, the reptilian type in the Dinosaur, and the mammalian type in the mastodon, the highest expressions of all of which belong to geologic periods, and whose living representatives, with few exceptions, belong to the humbler forms of life.

DEVELOPMENT IN PLANTS.

As a specialist only in the lower of the two great kingdoms it is not my place to enter into details respecting the working of these several laws in the animal kingdom, even if I were competent to do so. My illustrations must therefore be chiefly drawn from plants.

It is well known that the three principal groups of modern cryptogams, the ferns, Lycopodiaceæ, and Equisetaceæ, represent the degenerate descendants of a vegetation which formed extensive forests in Carboniferous time, and Hugh Miller, Dr. Lindley, and some more recent authors have used this fact in the manner above referred to, as demonstrating that the life-series of the globe is as likely to be a descending as an ascending one, and that development as a general principle is not proved. Of course it is now well understood that natural selection does not necessarily produce an ascending series, as for example, in parasitic degeneration. But the principle which I have formulated to-night of type degeneracy has been almost entirely ignored, although it is alone able to explain the most important facts that seem opposed to evolution in general. The modern degenerate cryptogamic vegetation is one of those facts and to it I must devote a few moments of explanation.

The so-called natural system of classification in botany is based primarily upon the reproductive function. As founded by Jussieu it was exclusively so based, but De Candolle undertook to introduce a new principle, viz., that of the structure of the axis or trunk, by which he separated exogenous from endogenous plants, and supposed that this line could be drawn between monocotyledons and dicotyledons, erroneously including the gymnosperms in the latter of these groups. The weight of his authority not only long retarded the discovery of the true position of the gymnosperms as the immediate descendants of the crytogams, but it had the further effect of barring out the important truth which vegetable paleontology has at last made clear that there once existed a large class of exogenous cryptogams.

Origin of Exogeny.—It has long been known that the Stigmarias of the coal measures possessed an exogenous structure, and as early as 1839 Brongniart discovered that the stems of *Sigillaria elegans* consisted of a medullary center surrounded by a thin exogenous zone within a thick cortex. The woody zone was shown to be composed of distinct wedges separated by medullary rays. It is now known that nearly or quite all coal plants having the external characters of Sigillaria have this exogenous zone. It was also early discovered that certain coal plants with the general appearance of Calamites exhibit an exogenous structure, and it was at first supposed that these must be something very different, and they were accordingly called *Calamodendron*. Professor Williamson has shown that all true calamites have an exogenous structure of a very definite character. He has also proved that the distinction drawn between Sigillaria and Lepidodendron based on this character is not tenable, since some true Lepidodendra also show the woody zone and medullary rays.

When Brongniart had made the discovery referred to he changed his mind with regard to the plants of the coal measures, and ever afterward maintained that Sigillaria and Calamodendron must be phanerogams, referring them to the Coniferæ. This complete reversal of his former logical and correct views was due to the preconceived opinion that exogenous growth was necessarily correlated with coniferous and dicotyledonous plants, as taught by De Candolle, and there is still a French school of vegetable paleontologists, who, as disciples of Brongniart, continue to maintain that Sigillaria must be placed in an entirely different class from Lepidodendron, and Calamodendron from Calamites, and who are disposed to deny the cryptogamic character of all forms possessing an exogenous structure.

Now the truth seems to be that in the process of development in plants the exogenous structure has been attained in varying degrees along several ascending lines, and that there is a different kind of exogeny in the calamite, the lepidophyte, the cycad, the conifer, and the dicotyledon, while something resembling exogeny has been shown to occur in certain fossil ferns and in certain living monocotyledons. Exogenous cryptogams probably no longer exist. The reign of the cryptogam has come to an end. It occurred in remote Carboniferous times when these plants constituted the greater part of the earth's vegetation. It was then that certain types of the Lycopodiaceæ and Equisetaceæ became forest trees and were supported by exogenous trunks. These types have long since disappeared according to the law of the extinction of trunk lines of descent, and it is only the earlier and simpler types that have come down to us according to the law of the persistence of unspecialized types. The filicine, equisetian, and lycopodian types continued to develop until they reached the highest state attainable by plants having that structure. They even acquired the exogenous character, but only in a rudimentary form.

It would be wholly misleading to place the exogeny of these plants on a par with that of the modern exogen. In the pine and the oak, as every one knows, the bulk of the trunk consists of what we call *wood*, that is, of concentric layers of thick-walled vascular cells, giving to the trunk great strength and resistance, and although in the great sequoias and in the cork-oak the cortical portion, or bark, may attain a thickness of over a foot, still this is a relatively small portion of the entire trunk, and contributes comparatively little to its support. Now, if we imagine a tree in which the bark constitutes the bulk of the trunk and the wood only a comparatively narrow zone close to the central pith, we shall have some idea of the exogenous cryptogamic forest tree of the Carboniferous age. Something approaching it can be seen on a small scale in the first year's growth of a modern exogen, and in most herbaceous plants of that type, and we have another approach to it in the trunks of living cycads.

But when we speak of such thick bark it must not be supposed that we mean the dry corky and flaky exterior which is popularly called bark. This, in the modern exogen, constitutes the greater part of the bark of old trees, but is really the cast-off and, to a greater or less extent, dead matter pushed outward by the annual growth of the bast and liber, or the true live bark of the tree. For every exogen is also an endogen outside of the cambium layer. The bark grows by the deposition of new matter to its interior. It was even so with the exogenous cryptogam, only the endogenous or cortical portion, *i. e.*, the bark, then constituted the greater part of the trunk, whereas it now forms only a thin zone at the periphery.

This difference of degree is so great that it practically amounts to a difference of type, and far back in early Carboniferous time the new type had begun to appear, seemingly along two independent lines, the one typified by the form called Noeggerathia and leading to the modern Cycadaceæ, the other by the form called Cordaites leading to the modern Coniferæ, which two great families rivaled each other for the mastery of the vegetable world during Mesozoic times.

Origin of Phænogamy—Gymnospermy.—It is, however, doubtful whether this great advance in the direction of strength and stability of trunk would have alone sufficed to give these new types the victory in the struggle with the tree-ferns, calamites, and lepidophytes of that epoch. Correlated with it was a still greater advance in the structure of their reproductive organs. The highest types of modern cryptogams only occupy the stage called *heterospory*, i. e., the possession of two kinds of spores, the microspore, or male, and the macrospore, or female spore. That stage was reached by all the higher types of Carboniferous cryptogams. But by a series of steps, which recent researches have enabled us to trace in living forms, the passage was made in that early day from heterosporous cryptogamy to true gymnospermy, and the barrier was crossed which separates the cryptogam from the phanerogam. The origin of true flowers, albeit they were minute, inconspicuous, and devoid of color, fragrance, or beauty, took place at that ancient date. They were some such flowers as our sago-palms and our pines and cedars have to-day. Many fruits have been preserved for us in the coal measures and some of them closely resemble those of the ginkgo or maiden-hair tree. There are other strong proofs that the earliest Coniferæ belonged to the yew tribe of the ginkgo type, a type which is now nearly extinct, having but this single living representative. It was this type, and not the true pines and firs, that represented the conifers during the Jurassic period when the cycadean vegetation predominated over all other forms. And yet this solitary survivor of that long line of ancestors, this waning, tottering, dying ginkgo, with its perfect nut and ample deciduous foliage, may be properly regarded as the highest type of conifers, while the pines, spruces, and junipers must be looked upon as somewhat lower types, persisting according to the law already explained.

Angiospermy.—The next great step was from gymnospermy to angiospermy, the beginnings of which are buried in obscurity. In the gymnosperm the tender developing ovule and maturing seed is exposed to every rude element that besets the life of a plant. Thus exposed it is impossible for it to attain that delicacy of organization necessary to the highest perfection of vegetable growth. Protection of the germ thus early became the great desideratum. When it was first attained we know not, but there are some uncertain indications that angiospermous plants existed in Carboniferous time. But if so they did not belong to the higher or exogenous types. The struggles for the protection of the trunk on the one hand, and for the protection of the germ on the other, were independent struggles. Progress toward exogeny had nothing to do with progress toward angiospermy, and if the latter was attained during Carboniferous or early Mesozoic time it was attained only by endogenous plants, and the earliest angiosperms were endogens and not exogens. That is, the lower type from the standpoint of internal structure became the higher type from the standpoint of floral structure. Progress could therefore only be slow. What was gained by the one was lost by the other. Not until both these steps should be taken by the same type of plants could any new departure take place, and the history of plants shows that it was not until this combination occurred that the great revolution in the vegetable world was brought about.

Exogenous Angiospermy.—The time came at last, we know not at what precise period, when exogenous plants acquired a closed ovary. This is the highest type of vegetation yet reached, and the proofs of its potency confront us every time we behold a modern forest of dicotyledonous trees. The great variety, beauty, strength, and grandeur of this now dominant vegetation amply attest the efficacy of exogeny combined with angiospermy in the attainment of vegetal perfection. Yet the time that elapsed from the beginning of either of these advances, taken alone, to that at which their fortunate combination took place was enormous. Not in the great coal period nor its closing Permian stage; not in the Trias which succeeded did there come forth a single exogenous plant whose germ was thus protected. The great and abundant fossil floras of the Rhetic and Lias of India, Australia, Bavaria, Sweden, and their near equivalents in Virginia and North Carolina, the Connecticut valley, and in both Old and New Mexico, have none of them yielded a trace of any such plant. The same is true of the equally abundant Oolitic floras of Yorkshire, France, Italy, Siberia, and Japan. Not even the highest Jurassic strata of any part of the world have with certainty produced an exogenous angiosperm. The oldest formation at which such plants occur is that on which our own city, the nation's capital, stands, viz., the Potomac formation, whose geological position is doubtful as yet, but if Jurassic, centainly represents the extreme uppermost part of that system. By the author of its flora, Professor Fontaine, it is regarded as the equivalent of the Wealden, which is now commonly supposed to be the fresh water equivalent of the Neocomian or lowest member of the Cretaceous. So late did this now overshadowing type of plant life appear upon the globe. The rapidity with which it advanced, conquering and supplanting all rivals, may be better understood when we remember that it forms eighty-five per cent. of the flora of the Dakota group, which corresponds to the Middle Cretaceous.

A new and vigorous type of vegetation had been developed, the genealogical vine had put forth a fresh branch, the plant world had acquired a new lease of life, and it seems to us, looking back over its history, to have actually taken a leap forward at about this epoch, and ever after to have marched on with enormous strides.

Development of Floral Envelopes.—The resources of improvement in organization were, however, not yet exhausted. The germ was, indeed, now protected, and might acquire within its safe chamber all the subtle shades of perfection possible, but the delicate floral organs by which the fecundation of this germ was accomplished were still exposed, as indeed, it would seem, to a greater or less extent, they must always be. Yet means of their better protection were possible and were gradually adopted.

Apetaly.—The very earliest flowers were probably destitute of any protecting envelopes (achlamydeous), and some such still exist, but most of the lowest types of dicotyledonous plants are provided with one floral envelope, sometimes reduced to a few mere scales, sometimes with several distinct sepals in a whorl around the essential organs, sometimes with these united at the base, and occasionally with a bell-shaped, funnel-shaped, or even tubular calyx. Such plants are called apetalous or monochlamydeous. Paleontology shows that those forms which are now apetalous, especially those in which the flowers are borne in catkins, or are nearly altogether naked, prevailed in early times over those provided with two sets of floral envelopes, which is far from being the case at present. Our law, too, is here again exemplified in the great perfection attained in those early times by such apetalous trees as the poplar, the plane-tree, the fig-tree, the laurel and the sassafras.

Polypetaly.—The next step was the development of a second floral envelope, which, however, had its beginnings in small, strap-shaped, or even bristle-shaped petals. In our current botanies as prepared by Gray, De Candolle, and Bentham and Hooker, the plants having separate petals, or polypetalous plants, such as the rose, buttercup, mallow, etc., are placed before those having the corolla all in one piece, like the morning-glory, honeysuckle, etc. This position is given them to indicate that the authors of these books regarded them as of higher rank. But the geological history of plants teaches that such, at least, was not the order of nature in their development. It shows that polypetalous plants were very early developed. We find them at the earliest epoch at which dicotyledons begin to appear in any great abundance. It is true that we rarely find the flowers, and cannot say with certainty that they were the same as they are to-day. It is quite possible that trees of Cretaceous time whose leaves resemble those of modern polypetalous genera may have then had wholly apetalous flowers, but this is as yet mere speculation.

In this group we have another fine illustration of the law which I have stated, according to which the highest attainable development of any given type of structure is early and rapidly acquired. We are in the habit of regarding our magnolias, our tulip-trees, and the Australian eucalypts, as among the finest specimens of polypetalous plants, and yet the genera Magnolia, Liriodendron, and Eucalyptus appear and are rather prominent in the Middle Cretaceous floras of Europe, Green-land, and America. There was some doubt until recently whether the Eucalyptus really was an American type, so remote is its present home. But during the past summer a member of this Society, Mr. David White, has conclusively demonstrated that these trees flourished in abundance on what is now Martha's Vineyard during the Cretaceous age. They probably extended over the entire western world in that vast antiquity before the human race had made its appearance on our planet.

Gamopetaly.—There was one other step to be taken, the step from the polypetalous to the gamopetalous flower, from a corolla consisting of numerous distinct petals forming a whorl around the stamens and pistil within the calyx, to a corolla consisting of a single piece in the form of a bell, a funnel, or a tube, more and more completely protecting the essential organs. The older botanies call such plants *monopetalous*, emphasizing the fact that the corolla is of one piece, but wholly ignoring the process by which it became so. In fact, by placing this group after the polypetalous one they suggest that they are lower in rank and that monopetalous plants may have become polypetalous by division of the corolla into numerous petals. The German investigators, however, have shown by embryological study that the movement has been in the other direction, the petals of polypetalous plants, having, as it were, united into a corolla, and this is confirmed by paleobotany in showing that polypetalous plants antedated monopetalous ones in the history of plant development. The later botanies, therefore, so far recognize this truth as to adopt the term *gamopetalous* to express this union or wedding of the petals.

The progress from polypetaly to gamopetaly had only begun when the geological record closed. Only a few gamopetalous fossil plants have been discovered. There is reason to believe that there were persimmons, whortleberries, olives, and arrow-woods, during Tertiary times, but most of these have small flowers, and in some of the living representatives the lobes of the corolla are cleft nearly to the base, suggesting that at an earlier period in their history they may have really been polypetalous. The more typical Gamopetalæ, with tubular or funnel-form corollas are for the most part unrepresented in the fossil state, and we must regard these plants as among the latest products of development in the vegetable kingdom.

Nature of Vegetal Development.—I have now endeavored to trace the progress of development in the vegetable kingdom from its earliest beginnings in cryptogamic life to its highest and latest expression in the gamopetalous dicotyledon, with a view especially to showing by what particular steps it has taken place, and how the two laws of the extinction of trunk lines of descent and the persistence of unspecialized types have combined to bring about the varied and abundant vegetation with which the earth is clothed. I have sought to emphasize the fact that this evolution has not been in a single ascending series, that the plants that have one after another succeeded to the mastery have each in turn attained the highest development possible to their respective types of structure and have then surrendered their sceptre forever to the new and more perfect types evolved from them, and have usually dwindled down to comparative insignificance but persisted on in some of their lowest forms. I have wished to make clear and patent the important but rather recondite and popularly little understood truth that biologic progress takes place through this sympodial dichotomy, and not by true dichotomy, much less by the ordinary monopodial branching represented by the common figure of a tree. In other words the phylogenetic tree is something considerably different from the common genealogical tree. It further and especially differs in being exceedingly irregular in the intervals of branching. Expressing the process in time we observe that vast periods pass in waiting for the working out of the most simple principle, which, when once hit upon, produces a complete and rapid revolution in an entire department of life. I can liken it in this respect only to the progress of mankind as brought about by great mechanical inventions made at irregular intervals and producing undreamed-of revolutions in the whole industrial frame-work of society. The length of the stationary periods in biologic evolution is determined by no fixed law. When a type of structure has advanced as far as it is capable of developing it remains stationary as long as nothing interferes with its continuance. If no change should take place in its environment it might continue for an indefinite period. As, by hypothesis, it can advance no farther it can only vary in the direction of deterioration or extinction. The type of structure once fixed can never change. Only the degree of vigor, luxuriance, or abundance can undergo modification. Deterioration is everywhere illustrated by the present cryptogamic vegetation. The Carboniferous forests of Lepidodendron and Calamites are represented by our little club-mosses and scouring rushes, although they must have descended from trunk lines which had not yet acquired the exogenous structure. Extinction is exemplified by the absence of exogenous cryptogams in the living flora, as also of most of the later cycadean and coniferous types. There are several interesting cases of partial and rapidly approaching extinction. Among such may be mentioned the maidenhair-tree, the mammoth and redwood trees, and also, it would seem, the tulip and plane trees, all of which in their turn dominated the vegetable kingdom, but now, though undiminished in vigor or structural perfection, have been restricted in range, reduced in number, and nearly crowded out of existence.

We have seen that the deterioration or extinction can be only brought about by a change of environment. The only cause for the predominance of a type is its greater adaptation to the existing environment. If undisturbed any given type of structure will equilibrate in the direction of greater adaptation until this is no longer possible. But complete adaptation, as I long ago pointed out,* is impossible. It is always possible for a new type to appear which shall respond more exactly to the surrounding conditions. The environment, it is true, may undergo unfavorable changes. The climate may change, or the type in its migrations may encounter unfriendly influences. Most effective of all is the ever-changing influence of the contemporary life with which a type must come into competition. It must, as we have seen, eventually encounter as a rival in the race for life, the new type which is to succeed it, endowed with elements of new life and with fresh powers both to overcome hostile influences and to utilize the resources of nature. Such superior types, as already shown, are ever and anon arising, proceeding from quarters least anticipated, appearing without regularity either as to place or time, springing sympodially from the original trunk, rising impiously above their parents, and ultimately overshadowing, repressing, crushing, and extinguishing the former lords of the vegetable kingdom. Such in brief is the generalized history of the rise and fall of empires in the world of plants.

What has thus far been said is perhaps sufficient to render clear to most minds the peculiar and complicated character of biologic evolution in general, and to show how widely it differs not only from the current crude popular conception of it, but also from the ideas which prevail among well informed and even scientific persons. I need not, I am sure, apologize in this age of specialists, for having confined myself almost exclusively to that kingdom of life with which I am most familiar. I believe that I can safely assume that the zoologists present, in whatever branch, have been able to parallel all the illustrations which I have given by similar ones in their own departments, leading to the same general conclusion.

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* American Naturalist, February 1881, p. 89.

EXTRA NORMAL DEVELOPMENT.

Thus far I have only taken account of what may be called the normal or legitimate causes of such advantageous modifications of structure as have resulted in the successive upward steps which organic life has taken in the course of its history. But there is another class which may be called extra-normal, abnormal, or even illegitimate causes. Normal or legitimate causes are such as result in the production of characters which are of direct use to the organism. In extra-normal or illegitimate causes the characters produced are such as have only an indirect effect. Thus in the vegetable kingdom normal development tends chiefly in the direction of strengthening the stem, increasing the foliar surface, and protecting the germ and reproductive organs, *i. e.*, in the direction of strength, nutrition, and reproduction, these being the three prime essentials of existence. The various modes of strengthening the trunk, and especially the attainment of complete exogeny, as seen in the trees of the present day, directly improved the conditions of existence and the chances for further development. The gradual attainment of broad appendicular expansions called leaves increased more and more the power to decompose the carbonic dioxide of the air which is the chief nourishment of plants. The separation of the sexes, the transition from spore-bearing to seed-bearing plants, the development of a closed ovary for the protection of the germ, and of floral envelopes for the protection of the stamens and pistils, all tended to perfect the reproductive function and render a higher type possible. These influences were therefore all normal and legitimate in acting directly upon the essential properties of the organism; and had no extra-normal or illegitimate influences come in to modify the results these direct ends would have been the only ones attained. Vegetation would doubtless have still been green as now, there would have been forests of large trees with strong solid trunks and umbrageous foliage; there would have been green grass and rushes, rank and luxuriant herbage, stately palms and graceful ferns, even as now, but this would have been all. Two of the leading features of the actual vegetation would have been wanting, viz., showy and fragrant flowers and highly colored, pleasantly flavored, and nutritious fruits.

A large, showy, or fragrant blossom is of no direct use to a plant. Indeed its nourishment is an expense to the normal growth of the plant. Still greater is the cost of the abundant nutritious matter in many fruits. In both these cases the value to the plant is indirect, and when we study the subject deeply we find that the cause of the development of such organs is a sort of teleological or final cause. Beautiful flowers and edible fruits are extra-normal or illegitimate products of nature, and those who fail to see this have but a crude and imperfect conception of the course of evolution.

Fortuitous Variation.—In a certain sense every influence that affects an organism is legitimate, and we have seen that the several great types have been brought into existence by the improvement of the special opportunities offered by the environment. We have also seen that these opportunities have presented themselves at long and irregular intervals, and, as it were, by chance. In this sense there is only a difference of degree between these normal and legitimate influences and those which I have called extra normal or illegitimate. Their occurrence was fortuitous. They were the result of accidental variations in an advantageous direction seized upon by nature for the creation of higher types of life.

There is a school of evolutionists who maintain that this is the only way in which progress takes place. This is held to be the strictly Darwinian view, as opposed to the Lamarckian view that the "appetencies," as Lamarck called them, *i. e.*, the individual efforts, strivings, and struggles of the organism in advantageous directions, aid in determining what the new and improved type shall be. In a paper which I had the honor to read before this society over a year ago on "Fortuitous Variation as illustrated by the genus Eupatorium"* I endeavored to show that this fortuitous variation was often successful even when no apparent advantage could result therefrom. The tendency to vary is in all directions, as from the center toward the surface of a sphere, and variation will take place in every direction which does not prove so disadvantageous as to render life impossible. In by far the greater number of cases the advantage or disadvantage is slight or imperceptible, and changes go on without improvement or deterioration, causing a great number of equally vigorous forms to arise, all differing more or less from one another. This accounts chiefly for the varied and manifold in nature, and but for this law, hitherto, so far as I am aware, unobserved, nature would be monotonous and uninteresting. From the esthetic point of view, therefore, this is the most important law of biology.

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* See abstract (all that was published) in Nature (London) for July 25, 1889 (Vol. XL, p. 310).

What is its importance from the scientific point of view? As you probably all know, there has been going on during several years past a very lively discussion of the principle of natural selection, and that principle has been vigorously attacked by a large and highly respectable class of working naturalists. Its vulnerable points have been fearlessly exposed and its defenders have been put to their wits' end to save it from serious impairment. It has seemed to me that their mode of defense was ill-chosen and that its weakness consisted in claiming too much for natural selection, more than it can justly be shown to accomplish. The weakest link in the chain is the first one, as Darwin himself admitted, and it seems strange that he, who maintained that the variations which natural selection seizes upon to the advantage of the organism are fortuitous, should not have conceived that these might go on as they begun for a long time and result in important changes that were neither beneficial nor injurious. Those who question the principle of natural selection insist with apparent justice that the incipient changes due to accidental variation during a single generation are utterly inadequate to perpetuate and multiply themselves, that their utility must be infinitesimal and practically nil; and they pertinently ask how the machinery of natural selection was ever set in motion. Strange as it may seem, the defenders of natural selection have thus far found no better answer to this argument than to deny its force and to maintain that every variation, however slight, if in the direction of utility, begins to operate from its inception and goes on increasing with cumulative strength. This answer is not satisfactory and its inadequacy has been sufficiently proved. It should be abandoned and some other substituted, and until this is done natural selection will continue to lack a solid basis upon which to rest.

But it seems to me that there is an answer to the objection, and one which fully meets it. This answer is nothing more nor less than the patent fact already stated that fortuitous variation actually does go on at all times, in many directions, and to great lengths, without any perceptible change in the degree of adaptation which the varying forms have to their environment. I have shown how this takes place in one important genus of plants, and it would be easy to extend the observation to almost any other genus. I doubt not that the animal kingdom is also full of examples.

Here then we have the solution of by far the worst difficulty in the way of natural selection. The beneficial effect need not be assumed to begin at the initial stage. It need not be felt until well-formed varieties have been developed without regard to any advantage in the particular differences which they present. There seems to be no flaw in this mode of solving this paramount problem, and if it is objected that it amounts to a new explanation of the origin of species, I am ready to admit it, and I believe that more species are produced by fortuitous variation than by natural selection. Natural selection is not primarily the cause of the origin of *species;* its mission is far higher. It is the cause of the origin of *types of structure*, such as those whose history I have endeavored to trace, and through which alone biologic evolution takes place.

Extra-Normal Influences in the Vegetable Kingdom.—Returning from this important digression to the subject of extra-normal influences in the vegetable kingdom, let us inquire more closely into their exact nature. As already remarked, the most important are those which have resulted in the development of beauty and fragrance in flowers and of bright colors and agreeable flavors in fruits. But these are by no means all, and we must thus account for most burs, spines, thorns, and other forbidding features, viscid and glandular hairs, as in the sundew, and irregular and peculiar forms of leaves, especially such as are seen in the pitcher-plants, and a great variety of other structures not connected with the reproductive function.

What then are these supra-normal or illegitimate causes which result in such peculiar products? In the first place they consist in special changes in the environment which are seized upon to the advantage of the plant. Plants in view of their stationary character, had especial need of two things, viz., *cross-fertilization* and *dissemination*. Growing together without power to change their position and mingle with remoter forms, there was perpetual danger that close interbreeding might deteriorate or destroy the stock. The seeds of such stationary organisms perpetually falling in the same spot tended to choke one another and to weaken and restrict the species. Every normal and legitimate means of averting these two dangers had been adopted by the earlier types of vegetation. The spores of cryptogams and the pollen of conifers were made so light that the winds would take them up and waft them to great distances. Certain grasses and other herbs were endowed with the peculiarity of being uprooted by the wind at the proper season and blown for miles over the plains, scattering their seeds. And even water had become and still remains a medium for the transportation of both pollen and seed from place to place and from shore to shore. But still these instrumentalities fell far short of the needs of the vegetable world in these directions. At last, and nearly at the same period in the earth's history, two new, and, one may almost say, unexpected agencies came forward, adapted respectively to the supply of these two prime necessities of the plant—viz., *insects* and *birds*.

Origin of Showy and Fragrant Flowers.—Away back in the dim darkness of the coal period when tree-ferns, calamites, and giant club-mosses, combined with archæ-typal yews to people the steaming swamps of a hot, cloud-laden island world, there existed a strange form of insect which can only be compared to the cockroaches of our day, but which seems to have embodied in its structure the beginnings of all the varied types of insect life, the promise and prophecy not only of our dragonflies and beetles, but also of our flies, bees, and butterflies. And during the long ages that followed, while the plant life was passing through the history which I have briefly sketched, the insect world was experiencing a similar unfolding, and new and improved types, very much as in plants, were coming into existence, attaining their maximum development, and giving way to still higher ones, until some time in the late Jurassic or early Cretaceous age forms began to appear which were adapted to obtain sustenance from the pollen, and perhaps from the stigmas of flowers. To do this they were obliged to pass from flower to flower and would unavoidably carry the dust that adhered to their heads, wings and feet from one flower to others more or less remote. Cross-fertilization, that "secret of Nature" discovered by Sprengel, was thus effected, and new vigor was instilled into those forms which for any reason had been so fortunate as to attract these winged friends. We can figure to ourselves a rivalry springing up among plants as to which should offer them the greatest inducement, and through the action of natural selection, which here found a typical field for its normal operation, the entire nature of flowers underwent a rapid change. To continue the figurative expression, all flowers vied to excel in beauty and attractiveness; for these tiny insects possess esthetic tastes which do not materially differ from those of mankind.

To size, showiness, and beauty of coloration, was often added fragrance which was especially successful with moths and other nocturnal insects. Many special inducements were held out. Sweet and nutritious nectars were secreted from the petals to lure on the unsuspecting creatures, and deep, and peculiar grooves, sacs, and spurs were developed to hold this nectar in large quantities. These nectaries were so adjusted that no bee could enter without passing directly over the stigma and brushing upon it the precious dust of other flowers. Wonderful contrivances thus came into existence to secure this supreme end of plant being, and the present world of flowers was ultimately evolved.

The profound modification accomplished by this agency was not confined to size, color, fragrance, and the secretion of nectar. The forms of flowers underwent in many cases a complete change, and an infinite number of wonderful irregularities appeared, varying from the slightest differences in the petals to the amazing abnormalities of the orchids, all calculated to adapt plants to the useful ministrations of insects, sometimes, as in the yucca, to those of a single species of insect without which reproduction is impossible.

And thus it has come about that the form of every flower has its special meaning which can be interpreted by those who have penetrated this great secret. We hear of the language of flowers—that the rose signifies beauty, the daisy innocence, the violet modesty, the myrtle love—but science has discovered a new and real language which the flower not only speaks but writes in clear characters, and which the botanist deciphers and reads by much the same methods that the assyriologist employs when he deciphers and reads the arrow-head inscriptions upon the tablets of Nineveh.

It is thus that flowers are accounted for by modern science in all their beauty and variety. The old idea that they were made for man to admire and enjoy is exploded, and yet it remains true that they were made to be admired and enjoyed by creatures capable of admiration and esthetic pleasure. It is not true that any flower was ever "born to blush unseen" or "waste its fragrance on the desert air." There is a standard of taste so universal that what pleases the bee, the ant, and the butterfly, also pleases the senses of man. Biology has overthrown the anthropocentric theory as astronomy has the geocentric, and every creature lives in and for itself and shares with man to some degree the sublime attributes of mind and soul.*

Origin of bright-colored and sweet-flavored fruits.—In seeking the origin of fruits we have to consider an almost parallel history of development to that which we have been studying in accounting for flowers. But here we must look to another kind of animal life, chiefly to the great family of birds. There were probably no bright-colored or sweet-flavored fruits until the close of Mesozoic time, because the future birds were as yet reptiles crawling over the ground or swimming in the waters, albeit some of them already possessed the inchoate attributes of their avian successors. Moreover, the vegetation of that early period was incapable of employing the intervention of winged life for its distribution. At first it consisted exclusively of spore-bearing plants whose dissemination was chiefly affected by the wind, and which depended upon the infinite multiplication of spores to make up for defective means of distribution. Later came on the gymnospermous types of cycadean and coniferous life, neither of which are now to any great extent adapted to the uses of the feathered world. Paleontology, both vegetable and animal, thus doubly confirms the view that fruits, in the sense here employed, had their origin simultaneously with the appearance of birds, as flowers did with that of flower-frequenting insects, toward the close of Mesozoic time. Attracted by their bright colors correlated with pleasant flavors, birds learned to visit the plants that bore such fruits. Flying thence to distant parts and voiding the hard seeds of berries and stones of drupes, they became the effective instruments for the dissemination of these forms.

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* Here and later on I use the term *soul* in the sense of conscious desire strong enough to induce active effort for its satisfaction.

The great problem of distribution was thus solved by bird life as was that of cross-fertilization by insect life, and just as plants vied with one another to attract insects to their flowers, so did they also vie with one another to attract birds to their fruits. Here again it was the universal esthetic faculty that enabled the ancient bird life to prepare the earth for human habitation, and yet, no more than in the previous case was man the final cause. So uniform is the standard of taste throughout the psychic world that what contributes to the pleasure of a bird or an insect also supplies some esthetic want in the race of men.

ABNORMALITIES OF SEX.

There is one other abnormal or supra-normal influence in the organic world which is so important and so well illustrates the principle now under consideration, that it seems proper briefly to advert to it. I refer to the causes which in many cases, particularly in the animal kingdom, make one sex differ so widely from the other.

An array of facts taken from asexual life and from the very early stages of sexuality converge to show that primarily and normally the female is the main trunk line of development, while the male is merely accessory, and need have no importance apart from the reproductive function. Such restriction actually exists in a great many of the lower organisms and in some that are quite highly organized, while throughout the invertebrate world the physical superiority of the female is the rule and that of the male is almost unknown. Female superiority is also the rule and male superiority the exception among all vertebrates except birds and mammals, and sometimes occurs even in these. Normal or legitimate development would make it universal. But in most birds and mammals, the opposite state of things exists, viz., male superiority, and we are so much more familiar with these two highest types of life that the impression is almost universal that the male sex is in some way the primary and dominant one. I shall not waste your time in attempting to refute this popular impression. Those who defend it simply display their lack of acquaintance with the lower forms of life. My own attention was drawn to the subject by certain remarkable phenomena presented by plants, but a study of the very early stages of animal life is sufficient, with the least reflection, to set the whole question at rest.*

The problem is, therefore, to account for this apparently abrupt reversal of the normal process of development as it went on prior to the advent of birds and mammals. What was the extraneous and illegitimate agency which began to operate early in the development of avian and mammalian life? The one term which most nearly expresses it is *sexual selection*, proposed by Darwin. In my opinion the discovery of the principle of sexual selection has equal if not higher rank than that of natural selection, since its influence when fully understood will be found to be as great, and to Darwin alone is due the entire credit of making it known. Strangely enough Dr. Alfred Russel Wallace, who simultaneously and independently worked out the law of natural selection, is disposed, as shown by his recent work on Darwinism, to reject sexual selection altogether as a factor in biology ; yet to my mind, it remains debatable which of these two great laws has exerted the more profound effects in modifying the course of organic development. It certainly cannot be said of natural selection that it has produced a complete revolution in that course, or has, so to speak, reversed the wheels of biologic progress, as sexual selection has done ; not in the sense of producing a retrograde movement, but in that of shifting the axis of evolution, if I may be allowed the expression, from its normal position to a wholly abnormal one, and raising to a prime factor what was originally a mere incident in the history of organic life.

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* For a fuller, though popular, treatment of this subject, see the *Forum* for November, 1888, Vol. VI, p. 266.

Female Selection.—But by sexual selection Darwin meant only *female selection*, which would be the more accurate expression. It was not until the era of birds and mammals that the female really began to exercise a choice, or if, as is proved in a few cases, the females of lower creatures did exercise a choice, the result was the same as in the higher, the superiority of the males.

You all understand this law too well to make any explanation of its operation necessary, and I only desire to bring it forward as one of the most important of all the abnormal or illegitimate influences that have brought about the present state of things. I also wish to point out its analogy to the other two influences which I have considered. For here again, size, strength, and beauty, as displayed in the males of so many animals and birds, are the products of a dawning and growing esthetic sentiment, the expression of a developing taste, which is so nearly identical with the most highly developed tastes of mankind that there are no higher objects of human admiration than the gorgeous plumage of birds or the graceful forms of animals—than, for example, the feathers of the ostrich or the antlers of the stag.

Male Selection.—The reign of female selection has been a long one, and throughout the two classes of animals in which it is chiefly displayed it still prevails in full force. It is probably still the dominant influence in the human race, even among its highest types, though here resulting more in mental than in physical superiority in men.

But there are signs that this may not always remain so. I long ago pointed out * that among the higher races of men a form of *male selection* has already begun to exert a strong influence. In civilized life the choosing is not left wholly to women, and with the progress of culture and refinement this mutuality of selection grows more and more marked. That male selection will prove equally effective with female selection is already proved by the ever increasing beauty of women under its influence ; and those who think men perverse because they prefer beauty to all other qualities, or women trivial because they make their personal appearance a leading aim of life, have never learned the great law of nature which overrules all the trite maxims of the purists, that beauty means worth—perfection—and that beautiful companions insure perfect offspring, an improved posterity, and a better and nobler race, of men as well as women. And this is why the love of and preference for the beautiful has a higher and a deeper sanction in the everlasting order of things than can be given by any church, any court of law, or any code of morals.

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* Dynamic Sociology, 1883, Vol. I, p. 613.

THE PSYCHIC ELEMENT.

In all the cases considered of what I have denominated extra-normal or illegitimate influences affecting the course of biologic evolution, there is revealed to the careful student a common principle to which their peculiar character is due; a certain element of power and independence which gives to them both their anomalous and eratic character among organic laws, and also their remarkable efficacy and success in accomplishing the ends of evolution itself. What is this common principle, this element of power? It is expressed in the single word *psychic*—I had almost said, in the one word *mind*. Philosophers correctly identify these conceptions, and anything that transcends the purely vital partakes of the attributes of mind. This new force, manifesting itself in at least three prominent ways at almost the same time in the earth's history, and producing such astonishing revolutions, was the psychic force beginning to respond to a long process of cephalization, or brain-enlargement, in the animal world. It represents the birth of the soul in nature; it was the response to a demand for the satisfaction of wants, of instincts, of tastes; it was the first expression of purpose and of will. For these are the attributes which led the bee to seek the nectar from the flower, the bird to visit the brilliant cluster of fruit, or the female of the higher creatures to choose the most beautiful male for its mate. And these are psychic qualities and represent the subjective half of the world of mind—the great heart of nature.

The strictly biologic record properly closes here. To show that this same force continues to produce its unlooked-for effects at a higher stage of development, operating from the objective side, through the intellect, or head of nature, and that the results have here been as much more surprising and far-reaching as the organisms through which they were accomplished were higher in the scale of development,* though an easy task, would not only carry me too far, but would trench upon the domain of anthropology and belong more properly to a sister society.

Cosmic Epochs.—Taking a retrospective view of the entire field of evolution and bearing in mind its uneven course as I have sought to depict it, there may be discerned, standing out prominently above all the minor fluctuations, a few great cosmic crises or epochs, in which the change appears so abrupt and so enormous as to suggest actual discontinuity. Three such cosmic epochs belong to the history of life on the globe. The first was the origin of life itself. The second was the origin of soul or will in nature. The third was the origin of thought or pure intellect. While I do not say that any of the factors producing these epochs came suddenly into existence, or that any definite lines exist separating life from soul or soul from intellect, theoretically speaking, the general fact remains that they are practically distinct principles, having diverse effects, originating at widely different periods in the earth's history, and succeeding one another in the order named. Of these three great principles, life, soul, and intellect, and of the cosmic epochs which they have produced, I have in the closing part of this address, attempted to consider the second only, and I have chosen it chiefly because its bearing upon evolution appears to have been wholly ignored or misunderstood. Soul or will is simply desire in the act of seeking satisfaction, and I once presented the evidence to show that this is a true natural force,† obeying all of the three Newtonian laws of motion; but its effects, compared with the other forces of cosmic and organic evolution, appear to us erratic or even spasmodic. Nevertheless its potency is far greater and the ends attained through it are upon the whole the same. It owes this character to the fact that it is a psychic force as distinguished from either physical or vital forces. Its study is therefore a part of psychology, and from it we should learn that psychology is simply a branch of biology and its study should begin with animals and not with man. Finally, the peculiar character of this psychic influence is due to its being a product of higher organization. Mind is to biology what protoplasm is to chemistry. Psychology is transcendental biology.*

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* This is the "indirect method of conation." See Dynamic Sociology, Vol. II, p. 99.

† Dynamic Sociology, Vol. II, p. 95.

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* So called by Auguste Comte, who refused to recognize it as a distinct science. See his Philosophic Positive, Vol. IV, p. 342.

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PROCEEDINGS

OF THE

BIOLOGICAL SOCIETY OF WASHINGTON

VOLUME VI.

FEBRUARY 8, 1890, TO DECEMBER 26, 1891.

WASHINGTON, D. C.
PRINTED FOR THE SOCIETY.
1892

PUBLICATION COMMITTEE.

CHARLES D. WALCOTT, *Chairman*.
R. E. C. STEARNS, F. H. KNOWLTON,
T. S. PALMER, F. V. COVILLE.

Proceedings of the Biological Society of Washington.

ERRATA FOR VOL. VI.

Page VIII., line 7, for *Theory to Evolution*, read *Theory of Evolution.*

Page IX., line 11, for *refuted*, read *discussed*, in the sentence: "Mr. Howard refuted Mr. Mann's statement."

Page XI., line 11, for *Pribylov*, read *Pribilof; idem* page XVI., line 21; page 73, second column, line 3; page 74, first column, line 21; page 77, first column, lines 35 and 38; second column, lines 10, 26, and 28.

Page XII., line 2, for *Tenth Annual*, read *Eleventh Annual Meeting.*

Page XVIII., line 5, for *Mojave*, read *Mohave.*

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LIST OF THE OFFICERS AND COUNCIL

OF THE

BIOLOGICAL SOCIETY OF WASHINGTON.

ELECTED JANUARY 10, 1891.

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VICE-PRESIDENTS.

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RICHARD RATHBUN,
C. D. WALCOTT.

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F. A. LUCAS.

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F. W. TRUE,
GEORGE VASEY,
C. D. WALCOTT,
LESTER F. WARD,*
C. A. WHITE,*
C. V. RILEY.

STANDING COMMITTEES—1891.

Delegates to the Joint Commission of Scientific Societies of Washington.

C. HART MERRIAM,
LESTER F. WARD,
RICHARD RATHBUN.

Committee on Communications.

RICHARD RATHBUN, *Chairman.*
W. B. BARROWS,
JOHN MURDOCH.

Committee on Publications.

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Committee on Trees and Shrubs.

LESTER F. WARD, *Chairman.*
GEORGE VASEY,
F. H. KNOWLTON,
THEODORE HOLM,
F. V. COVILLE.

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* Ex-Presidents of the Society.

PROCEEDINGS.

ONE HUNDRED AND FIFTY-FIFTH MEETING,
February 8, 1890.

The President in the chair, and thirty-eight members and guests present.

The following active members were elected on recommendation of the Council: T. W. Stanton, U. S. Geological Survey; Dr. E. Roome, Columbian University.

Dr. Frank Baker presented a communication upon AN UNDESCRIBED MUSCLE FROM THE INFRACLAVICULAR REGION OF MAN. Discussed by Drs. Merriam and Riley.

Mr. C. D. Walcott presented a note upon A NEW GENUS AND SPECIES OF OSTRACOD CRUSTACEAN FROM THE LOWER CAMBRIAN.*

Dr. Cooper Curtice read a paper on THE MOULTINGS OF THE CATTLE TICK (*Ixodes bovis*).† Discussed by Drs. Riley and Theobald Smith.

Prof. Lester F. Ward spoke of FLOWERS THAT BLOOM IN THE WINTER TIME.

ONE HUNDRED AND FIFTY-SIXTH MEETING,
February 22, 1890.

The President in the chair, and thirty members present.

Mr. F. V. Coville presented a paper on THE NEW ARRANGEMENT OF GENERA IN THE HERBARIUM OF THE DEPARTMENT OF AGRICULTURE.‡ Discussed by Messrs. Riley and Ward.

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* Tenth Annual Report of the Director U. S. Geol. Survey, Pt. I, 1890, pp. 625, 626.

† Journal Comp. Med. and Vet. Archives 1890, p. 313; reprinted in Veterinarian 1891, p. 680.

‡ Bot. Gazette, xv, 1890, p. 68.

Dr. T. H. Bean presented NOTES ON SOME FISHES FROM BRITISH COLUMBIA.*

Dr. Merriam read a paper on the EVIDENCE OF SONORAN ORIGIN OF THE FLORA AND FAUNA OF THE GULF STATES. Discussed by Messrs. Ward, Gilbert, Coville, and Dall.

ONE HUNDRED AND FIFTY-SEVENTH MEETING,

March 8, 1890.

The President in the chair, and forty-one members present.

Mr. B. T. Galloway read a paper giving the results of some observations on an apple disease caused by the fungus *Gymnosporangium macropus*. Discussed by Messrs. Van Deman and Marlatt.

Mr. C. L. Hopkins presented some NOTES ON THE ANIMAL LIFE ABOVE THE SNOW LINE ON MT. SHASTA.† Discussed by Messrs. Mann, Riley, Howard, Diller and Hasbrouck.

Dr. W. H. Dall read a paper entitled ON DYNAMIC INFLUENCES IN EVOLUTION.‡ Discussed by Messrs. Coville, Mann, Fernow and Ward.

ONE HUNDRED AND FIFTY-EIGHTH MEETING,

March 22, 1890.

The President in the chair, and thirty-nine members present.

Dr. C. H. Stowell was elected an active member.

Dr. D. W. Prentiss read a paper entitled CHANGE IN THE COLOR OF HUMAN HAIR,§ CHANGE IN THE COLOR OF PLUMAGE OF BIRDS, AND IN THE FUR OF MAMMALS.||

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* Proceedings U. S. Nat. Mus., Vol. xii, pp. 641, 642.

† Insect Life, Vol. 2, p. 355.

‡ Proc. Biological Society, Vol. vi, pp. 1–10.

§ Phila. Med. Times, xi, 1880–81; also, Therap Gazette, Detroit, Mich., 1889, viii.

|| Journal Amer. Med. Assoc. xiii, 1889.

Prof G. B. Goode read a paper on the COLORS OF FISHES.*

Prof. C. V. Riley spoke on the COLORS OF INSECTS.

The papers were briefly discussed by Messrs. Mann, Dall, Seaman and Ward.

ONE HUNDRED AND FIFTY-NINTH MEETING,

April 5, 1890.

The President in the chair, and twenty-five members present.

Dr. Theobald Smith read a paper entitled SOME ILLUSTRATIONS OF FERMENTS AND FERMENTATION AMONG BACTERIA.† Discussed by Professor Atwater.

Dr. R. R. Gurley presented a paper on the AMERICAN GRAPTOLITES.

ONE HUNDRED AND SIXTIETH MEETING,

April 19, 1890.

The President in the chair, and forty-five members present.

Dr. C. W. Richardson was elected an active member of the Society.

Dr. Merriam presented a paper entitled HISTORICAL REVIEW OF THE FAUNAL AND FLORAL DIVISIONS THAT HAVE BEEN PROPOSED FOR NORTH AMERICA. Discussed by Dr. Dall.

Prof. J. F. James read a paper entitled VARIATION WITH ESPECIAL REFERENCE TO CERTAIN PALEOZOIC GENERA. Discussed by Professor Ward.

Dr. Dall exhibited some ORIGINAL DRAWINGS OF THE FUR SEAL AND STELLERS' SEA COW,‡ and gave an account of them.

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* Trans. Am. Fisheries Society, 1890.

† Centralblatt für Bakteriologie u Parasitenkunde, vii, 1890, p. 502.

‡ Report U. S. Coast Survey and Geodetic Survey, 1890.

ONE HUNDRED AND SIXTY-FIRST MEETING,
May 3, 1890.

The President in the chair, and thirty-six members present.

Mr. J. R. Edson was elected an active member.

Dr. Robert Reyburn read a paper entitled THE LIFE HISTORY OF MICRO-ORGANISMS WITH ITS RELATION TO THE THEORY TO EVOLUTION.* Discussed by Dr. Theobald Smith, Dr. Salmon, Mr. True, Mr. Erwin Smith, Dr. Shaeffer, Mr. Seaman and Dr. Dall.

Dr. Vasey read a paper entitled A NEW GRASS GENUS.† Discussed by Messrs. Holm, Coville and Prof. Ward.

ONE HUNDRED AND SIXTY-SECOND MEETING,
May 17, 1890.

The President in the chair, and thirty-six members present.

Mr. T. S. Palmer read a paper entitled SOME EARLY VIEWS OF GEOGRAPHICAL DISTRIBUTION OF SPECIES. Discussed by Dr. Merriam.

Mr. F. W. True exhibited a specimen of *Lophiomys imhausii*. Discussed by Dr. Merriam.

Prof. W. H. Seaman read a paper entitled THE PLACE OF BIOLOGY IN THE PUBLIC SCHOOLS.‡ Discussed by Profs. Chickering and Ward, Dr. Baker and Messrs. Waite and Howard.

ONE HUNDRED AND SIXTY-THIRD MEETING,
May 31, 1890.

The President in the chair, and twenty-six members present.

Dr. C. H. Merriam exhibited specimens of sundry new species of NORTH AMERICAN MAMMALS. Discussed by Dr. Gill.

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* American Monthly Microscopical Journal, June, 1890.

† Bot. Gazette, xv, 1890, p. 106.

‡ The American Anthropologist for October, 1891.

Dr. Theodore Gill presented a paper on the CHARACTERISTICS OF THE HALOSAUROID FISHES.*

Dr. J. N. Rose read a paper entitled *Coulterella*, A NEW GENUS OF COMPOSITÆ.† Discussed by Messrs. Seaman, Vasey and T. S. Palmer.

Prof. Jos. F. James spoke on ORGANISMS IN ST. PETER'S SANDSTONE.

Mr. B. P. Mann made some remarks upon the AUTHORSHIP OF THE BIBLIOGRAPHY OF ECONOMIC ENTOMOLOGY, published by Department of Agriculture.

Mr. Howard refuted Mr. Mann's statement.

ONE HUNDRED AND SIXTY-FOURTH MEETING,
October 18, 1890.

The President in the chair, and twenty-eight members present.

Mr. H. E. Van Deman spoke of CULTIVATED FRUITS IN THE MOUNTAINS OF NORTH CAROLINA.‡ Discussed by Dr. Salmon.

Dr. Theo. N. Gill presented a communication on the SUPER FAMILY CYCLOPTEROIDEA.§

Prof. Lester F. Ward spoke on the subject of AMERICAN TRIASSIC FLORA.||

ONE HUNDRED AND SIXTY-FIFTH MEETING,
November 1, 1890.

The President in the chair, and thirty-seven members present.

Mr. Nathan Banks was elected an active member.

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* American Naturalist, xxiii, 1889, pp. 1015, 1016. (Pub. May, 1890.)

† U. S. Cont. Nat. Herb. i, 1890, p. 71.

‡ Ann. Report U. S. Dept. Agriculture, 1890, pp. 410, 411.

§ Proc. U. S. N. M. xiii, 1890, pp. 361–376; pl. 28–30.

|| Geological Society of America, iii, 1891, pp. 23–31. Abstracts in Science, Vol. xviii, Nov. 20, 1891, pp. 287, 288, and Proc. A. A. A. S., Vol. xi.

Mr. F. V. Coville read a paper entitled FRUITING OF THE GINKGO AT THE DEPARTMENT OF AGRICULTURE. Discussed by Dr. Riley, Mr. Seaman and Mr. Fernow.

Dr. Marx spoke of his recent INVESTIGATIONS OF THE POISON GLANDS OF LATHRODECTUS. Discussed by Drs. Riley, Dall and Theobald Smith.

Prof. Jos. F. James read a paper called FUCOIDS AND OTHER PROBLEMATIC ORGANISMS. Discussed by Mr. Lucas, Dr. Dall, Prof. Ward and Mr. Mann.

ONE HUNDRED AND SIXTY-SIXTH MEETING,
November 15, 1890.

The President in the chair, and forty-three members present.

Dr. Merriam presented a communication entitled LIFE IN THE LAVA BEDS AND CAÑONS OF SNAKE RIVER, IDAHO, IN OCTOBER.* Discussed by Messrs. Walcott and Howard.

Mr. Theodore Holm spoke of the VEGETATIVE PROPAGATION OF DICENTRA CUCULLARIA.†

Dr. Dall presented some PALEONTOLOGICAL NOTES FROM THE NORTHWEST COAST.‡

Mr. Lucas described a FOOT DISEASE OF CAPTIVE BIRDS.

ONE HUNDRED AND SIXTY-SEVENTH MEETING,
November 29, 1890.

The President in the chair, and forty-seven members present.

The following new active members were elected: Jno M. Stedman, Merwin M. Snell and Rev. Alexis Orban.

Dr. T. H. Bean presented a paper on THE DEATH OF SALMON AFTER SPAWNING.§ Discussed by Dr. Gill, Prof. Goode, Mr. Stejneger, Drs. Dall and Merriam.

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* (In part.) North American Fauna No. 5, July, 1891, pp. 6–7.

† Bull. Torrey Botanical Club, Vol. xviii, 1891, pp. 1–11, pl. cxi-cxiii.

‡ Nautilus, Philadelphia, Vol. iv, 1890, No. 8, pp. 87–89, December.

§ Forest and Stream, November 27, 1890.

Dr. Theobald Smith spoke ON SPECIES AMONG BACTERIA. Discussed by Mr. True, Dr. Gill, Drs. Riley, Curtice, Mr. Banks.

Mr. Sudworth presented a communication entitled NOTES ON NOMENCLATURE. Discussed by Dr. Merriam.

ONE HUNDRED AND SIXTY-EIGHTH MEETING.

December 13, 1890.

The President in the chair, and twenty-five members present.

Mr. A. B. Baker was elected an active member.

Mr. Wm. Palmer read a paper on THE OCCURRENCE OF THE ASIATIC CUCKOO ON THE PRIBYLOV ISLANDS. Discussed by Dr. Dall.

Dr. Riley presented some NEW NOTES ON THE GENUS PHYLLOXERA.

Mr. True spoke on THE TEETH OF THE MUSKRAT.

Mr. Lucas read a paper on THE WING OF METOPIDIUS. Discussed by Mr. True.

ONE HUNDRED AND SIXTY-NINTH MEETING,

December 27, 1890.

The President in the chair, and twenty-six members present.

The following active members were elected: J. M. Holzinger, Frederick C. Test.

Dr. Cooper Curtice presented a communication entitled A PRELIMINARY STUDY OF TICKS IN THE UNITED STATES. Discussed by Dr. Smith.

Dr. C. Hart Merriam exhibited A NEW RABBIT FROM THE SNAKE PLAINS OF IDAHO.*

Dr. W. H. Dall read a paper entitled ON THE TOPOGRAPHY OF FLORIDA WITH REFERENCE TO ITS BEARING ON FOSSIL FAUNAS.†

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* (*Lepus idahoensis*) North American Fauna No. 5, July, 1891, pp. 76–78.

† Bulletin 84, U. S. Geol. Survey. (In press.)

ONE HUNDRED AND SEVENTIETH MEETING,
(Tenth Annual.)

The President in the chair, and forty-two members present.

The annual reports of the Secretary and Treasurer were read and accepted.

The following board of officers was elected for the ensuing year:

President—C. Hart Merriam.

Vice-Presidents—C. V. Riley, Frank Baker, Richard Rathbun, and C. D. Walcott.

Secretaries—L. O. Howard and F. A. Lucas.

Treasurer—F. H. Knowlton.

Members of Council—F. W. True, T. H. Bean, R. E. C. Stearns, Theobald Smith, and Geo. Vasey.

ONE HUNDRED AND SEVENTY-FIRST MEETING,
January 26, 1891.

(Eleventh Anniversary Meeting.)

The eleventh anniversary meeting was held in the law lecture-room of Columbian University, January 26, 1891, in the presence of members and invited guests.

The President, Lester F. Ward, delivered his annual address on the subject NEO-DARWINISM and NEO-LAMARCKISM.*

ONE HUNDRED AND SEVENTY-SECOND MEETING,
February 7, 1891.

The President in the chair, and twenty-five members present.

The President announced the following committees for the ensuing year:

Joint Commission—C. Hart Merriam, Lester F. Ward, Richard Rathbun.

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* Published in this volume. See pp. 11–71.

Committee on Communications—Richard Rathbun, Walter B. Barrows, John Murdoch.

Committee on Publications—C. D. Walcott, R. E. C. Stearns, F. H. Knowlton, T. S. Palmer, F. V. Coville.

Committee on Trees and Shrubs—Lester F. Ward, Geo. Vasey, F. H. Knowlton, Th. Holm, F. V. Coville.

Mr. C. D. Walcott presented a paper on the DISCOVERY OF VERTEBRATE LIFE IN LOWER SILURIAN (Ordovician) STRATA.* Discussed by Dr. Gill.

Prof. Henry F. Osborn gave a REVIEW OF THE DISCOVERY OF CRETACEOUS MAMMALS.† Discussed by Dr. Gill.

ONE HUNDRED AND SEVENTY-THIRD MEETING,
February 21, 1891.

The President in the chair, and twelve members present.

Dr. Cooper Curtice read a paper entitled SOME LITTLE KNOWN WORMS IN CATTLE. Discussed by Mr. Holzinger.

ONE HUNDRED AND SEVENTY-FOURTH MEETING,
March 7, 1891.

The President in the chair, and twenty-five members present.

Mr. F. A. Lucas exhibited and described some young HOATZINS. Discussed by Dr. Dall.

Mr. Lucas also drew attention to a SPECIMEN OF BISON LATIFRONS FROM PEACE CREEK, FLORIDA.

Dr. Bean spoke of the FISHES OF GREAT SOUTH BAY.‡

Mr Rose spoke of A NEW ASTER FROM SOUTHERN CALIFORNIA.§ Discussed by Mr. Holzinger and Mr. Waite.

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*Bull. Geol. Soc. Amer. Vol. iii, 1891 (in press). See also Proc. Phila. Acad.

† American Naturalist, xxv, July, 1891, pp. 295-611. See also Proc. Phila. Acad.

‡ Nineteenth Report of the Commission of Fisheries of New York, 1891, pp. 237–281.

§ Bot. Gazette, Vol. xvi, 1891, p. 113.

Mr. Sudworth presented a communication on the COLOR AND ODOR OF FLOWERS IN ATTRACTING INSECTS. Discussed by Messrs. Howard and Marlatt.

Mr. Stedman exhibited and described a fine specimen illustrating the EMBYRO OF A CHICK WITH TWO PROTOVERTEBRÆ.

Dr. Merriam spoke of DISTRIBUTION OF ANIMAL AND VEGETABLE LIFE. Discussed by Dr. Curtice and Messrs. Waite, Test and Holzinger.

ONE HUNDRED AND SEVENTY-FIFTH MEETING,

March 21, 1891.

Ex-President Ward in the chair, and twenty-eight members present.

Mr. W. A. Taylor was elected an active member.

Dr. Dall presented a paper on the AGE OF THE PEACE CREEK BONE BEDS IN FLORIDA.*

Dr. Shufeldt read a communication on A COLLECTION OF FOSSIL BIRDS FROM THE EQUUS BEDS OF OREGON.†

Mr. F. A. Lucas spoke of A POINT IN THE ANATOMY OF HESPERORNIS.

Mr. F. H. Knowlton presented a communication entitled WHAT ARE CYPRESS KNEES? Discussed by Prof. Ward and Mr. R. L. Garner.

ONE HUNDRED AND SEVENTY-SIXTH MEETING.

April 4, 1891.

Vice-President Walcott in the chair, and thirty-two members present.

Dr. Cornelius B. Boyle was elected an active member of the Society.

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* Bulletin 84, U. S. Geol. Survey. (In press).

† To be published in the Acad. of Nat. Sciences of Phila. Abstracts <Amer. Nat. Sept. 1891, pp. 818–821. The Auk, Vol. viii, Oct., 1891, pp. 365–368.

Dr. Bean read a paper on KENNERLY'S SALMON.* Discussed by Dr. Dall, Mr. C. D. Walcott and Mr. Waite.

Dr. Theobald Smith made some REMARKS ON RECENT BACTERIOLOGICAL PROGRESS IN THE PREVENTION AND CURE OF DISEASE.

Dr. V. A. Moore spoke of the PRODUCTION OF IMMUNITY IN GUINEA PIGS WITH STERILIZED CULTURES OF HOG CHOLERA BACILLUS. Discussed by Drs. Curtice, Schaeffer and Salmon, and Mr. Walcott.

Mr. E. M. Hasbrouck read a MONOGRAPH OF THE CAROLINA PARRAKEET.†

ONE HUNDRED AND SEVENTY-SEVENTH MEETING,

April 18, 1891.

Vice-President Riley in the chair, and twenty members present.

Prof. L. H. Dewey, was elected an active member of the Society.

Mr. Van Deman presented a communication on THE RECENT INTRODUCTION OF DATE PALMS.‡

Dr. Curtice read a paper entitled PRACTICAL VALUE OF INVESTIGATING PARASITES OF LIVE STOCK.

Prof. Lester F. Ward spoke of SOME FLORIDA PLANTS.

ONE HUNDRED AND SEVENTY-EIGHTH MEETING,

May 2, 1891.

Vice-President Riley in the chair, and twenty-eight members present.

Dr. Gill spoke of the CLASSIFICATION OF THE APODAL FISHES.§ Discussed by Mr. Lucas.

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* Forest and Stream, July 9, 1891.

† The Auk, Oct., 1891, pp. 369–379.

‡ Ann. Report, U. S. Dept. of Agriculture, 1890, p. 415.

§ Proc. U. S. Nat. Museum, Vol. xiii, 1890, pp. 157-242.

Mr. Galloway read a paper entitled RECENT PROGRESS IN THE STUDY OF PLANT DISEASES. Discussed by Dr. Erwin F. Smith.

Dr. Frank Baker presented some NOTES ON DWARFS. Discussed by Messrs. Ward, Gill and Riley.

Mr. F. A. Lucas read a paper, by Mr. Chas. Hallock, entitled DISTRIBUTION OF FISHES BY UNDERGROUND WATER COURSES.* Discussed by Dr. Gill.

Mr. F. C. Test presented some NOTES ON THE DENTITION OF DESMOGNATHUS.

ONE HUNDRED AND SEVENTY-NINTH MEETING,

May 16, 1891.

Vice-President Riley in the chair, and twenty-two members present.

Prof. Riley read a paper on THE MEXICAN ARROW WEED AND JUMPING BEAN.† Discussed by Dr. J. N. Rose, Prof. Ward, and Dr. Vasey.

Mr. J. M. Holzinger read a paper entitled INCENTIVES TO NATURAL HISTORY STUDY.

Mr. Wm. Palmer described THE DISTRIBUTION OF CERTAIN MAMMALS, BIRDS AND PLANTS ON THE PRIBYLOV ISLANDS.

Dr. Vasey read some NOTES ON THE RECENT FIELD WORK OF THE BOTANICAL DIVISION OF THE DEPARTMENT OF AGRICULTURE.

Mr. Lucas presented some REMARKS ON A NEW TORTOISE FROM THE GALAPAGOS ISLANDS.‡

ONE HUNDRED AND EIGHTIETH MEETING,

October 17, 1891.

The President in the chair, and fifty-three members present.

Mr. W. T. Swingle was elected an active member of the Society.

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* Amer. Angler.

† Scient. Amer., June 13, 1891; Amer. Nat., July, 1891, pp. 673–675.

‡ Nature, June 4, 1891, p. 113.

Mr. F. V. Coville read a paper entitled FOOD PLANTS OF THE INDIANS OF THE DEATH VALLEY REGION. Discussed by Mr. Van Deman, Dr. Merriam, and Dr. Vasey.

Dr. R. W. Shufeldt presented some NOTES ON PALEOPATHOLOGY.* Discussed by Mr. Lucas and Mr. Gilbert.

Mr. Wm. Palmer read a paper on THE FATE OF THE FUR SEAL IN AMERICAN WATERS.† Discussed by Dr. Dall, Dr. Merriam, Mr. Lucas, Dr. Bean, and Dr. Schaeffer.

ONE HUNDRED AND EIGHTY-FIRST MEETING,
October 31, 1891.

The President in the chair, and thirty members present.

The following were elected active members of the Society: Mr. A. G. Masius, Prof. B. W. Everman, Dr. W. F. Morsell, Dr. C. W. Stiles.

Dr. Theo. Gill presented a communication on THE CLASSIFICATION OF THE TETRAODONTOIDEA.‡

Dr. T. H. Bean made some remarks on SOME FISHES NEW TO NEW ENGLAND WATERS.§ Discussed by Dr. Gill and Mr. Barrows.

Mr. W. B. Barrows read a paper on CUCKOO STOMACHS AND THEIR CONTENTS.

Dr. N. H. Egleston read a paper on THE TEMPERATURE OF TREES.

Dr. C. W. Stiles presented some NOTES ON PARASITES—THE DEVELOPMENT OF ECHINORHYNCHUS GIGAS.|| Discussed by Mr. Howard and Dr. Merriam.

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* The Popular Science Monthly, 1892.

† Forest and Stream, Oct. 29, 1891.

‡ Proc. U. S. Nat. Museum, xiv, 1891.

§ Forest and Stream, Dec. 17, 1890.

|| American Journal for Comp. Med. and Vet. Archives, Dec., 1891. French translation in Comp. rend. de la Soc. de Biologie, Paris, 1891, pp. 764-766.

ONE HUNDRED AND EIGHTY-SECOND MEETING,
November 14, 1891.

The President in the chair, and thirty-one members present.

Mr. T. S. Palmer read a paper entitled THE WINTER ASPECTS OF THE MOJAVE DESERT REGION.

Dr. V. A. Moore spoke of a case of ECHINOCOCCUS IN SWINE.* Discussed by Drs. Smith and Stiles.

Dr. C. W. Stiles presented a paper entitled NOTES ON PARASITES, describing *Coccidium bigeminum* and *Filaria gasterostei.*†

Prof. Lester F. Ward spoke of HAECKEL'S RADIOLARIA OF THE CHALLENGER EXPEDITION, and presented a communication entitled THREE DAYS IN THE TROPICS. The latter was discussed by Dr. Merriam and Mr. Coville.

ONE HUNDRED AND EIGHTY-THIRD MEETING,
November 28, 1891.

The President in the chair, and thirty-six persons present.

The following active members were elected on recommendation of the Council:

Mr. W. E. Clyde Todd and Mr. J. D. Figgins.

Dr. George Marx read a paper on THE STRUCTURE AND CONSTRUCTION OF THE GEOMETRIC SPIDER WEB. Discussed by Dr. Th. Gill.

Mr. David White presented a paper entitled SOME PECULIAR FORMS IN AN UPLAND CARBONIFEROUS FLORA.‡ It was discussed by Prof. Lester F. Ward.

Prof. F. H. Knowlton presented a paper on FRUITING FERNS FROM THE LARAMIE GROUP. Discussed by Professor Ward, Mr. C. D. Walcott and Dr. Cooper Curtice.

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* Amer. Journal of Comp. Med. and Vet. Archives and Annual Report of the Dept. of Agriculture, 1891. Report on Animal Parasites for 1891, C. W. Stiles.

† Proc. U. S. Nat. Museum, 1892. Note preliminarie sur quelques Parasites, Bull. d. l. Soc. Zool. d France, 1891, pp. 163-165.

‡ Bulletin 98, U. S. Geological Survey. (In press.)

ONE HUNDRED AND EIGHTY-FOURTH MEETING,
December 12, 1891.

The President in the chair, and thirty-eight members present.

Mr. F. V. Coville read a paper entitled A REVIEW OF KUNTZE'S REVISIO GENERUM PLANTARUM. Discussed by Mr. J. N. Rose and Dr. C. Hart Merriam.

Mr. E. M. Hasbrouck presented some REMARKS ON DICHROMATISM. Discussed by Dr. W. H. Dall, Dr. Erwin F. Smith, Dr. C. Hart Merriam, and Mr. F. W. True.

Prof. Lester F. Ward spoke of RECENT DISCOVERIES OF POTOMAC FOSSIL PLANTS NEAR WASHINGTON.

ONE HUNDRED AND EIGHTY-FIFTH MEETING,
December 26, 1891.

The President in the chair, and twenty-six members present.

Mr. F. H. Knowlton presented a paper entitled A FOSSIL BREAD-FRUIT TREE FROM THE SIERRAS OF CALIFORNIA. Discussed by Prof. Ward.

Prof. Lester F. Ward presented a communication entitled ALPHONSE DE CANDOLLE ON THE TRANSMISSION OF ACQUIRED CHARACTERS.

Dr. C. Hart Merriam made extended REMARKS ON THE AFFINITIES OF THE NORTH AMERICAN SQUIRRELS, CHIPMUNKS, SPERMOPHILES, PRAIRIE DOGS, AND MARMOTS. Discussed by Dr. Erwin F. Smith, Dr. Stiles, Dr. Dall, and Professor Ward.

ON DYNAMIC INFLUENCES IN EVOLUTION.

BY WM. H. DALL.*

It is generally admitted that in the doctrine of Natural Selection we have a theory which accounts for the perpetuation of favorable variations in organic beings and their progeny, and for the elimination in the long run of those which vary in unfavorable directions. It is equally admitted that the origin of variation is not accounted for by this theory. In order to round out our conception of the mode of evolution of the organic universe it is necessary that this deficiency should be supplied, and that to it should be added some conception of the mode by which variation is sustained in any given direction until it has reached a point where its usefulness is sufficiently marked to enable the selective process to operate. Besides this it is hardly doubtful that there are many characters developed in organisms, especially those of the lower rank, in which selection of any sort is but little concerned.

It is not necessary to recapitulate the names of those who have turned to the relations between the organism and its environment as the only nidus of the influences sought. Such an enumeration would comprise nearly all American biologists of prominence and many foreign naturalists.

On the other side of the Atlantic a small but not unimportant number of biologists, of whom Weismann and Lankester may be taken as spokesmen, have recently endeavored to show that the current of hypothesis most favored in America, though not confined to our naturalists, is running in a wrong direction, although they do not seem to have any satisfactory alternative to offer.

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*Read before the Biological Society of Washington, Mar. 8, 1890.

For convenience in discussion those who accept the ideas referred to, in greater or less degree, may be termed Dynamic Evolutionists. Their position has been very fairly and temperately stated by Osborne in his article on the paleontological evidence of the transmission of acquired characters.* Without attempting to speak for others I have felt that a statement of the position to which I have been led by my own studies might not be without use in the present status of the question.

In the first place, in opposition to the notion that characters acquired in other than the embryonic or larval condition are not transmitted to the progeny ;—I maintain that a direct or indirect transmission of acquired characters is absolutely essential to any theory of evolution and that, speaking broadly, the whole system of Darwinism must stand or fall with this hypothesis. It is as axiomatic as the "survival of the fittest" itself.

It therefore becomes necessary to define what is meant by "acquired characters" and their "transmission."

The environment stands in a relation to the individual such as the hammer and anvil bear to the blacksmith's hot iron. The organism suffers during its entire existence a continuous series of mechanical impacts, none the less real because invisible, or disguised by the fact that some of them are precipitated by voluntary effort of the individual itself. So far as results are concerned, for the ground to strike the horse's hoof would be the same as for the horse to strike the ground with his hoof; direction and dynamic value of shock being assumed to be equal in the two cases. Since individual organisms usually appear free to wander about or remain quiescent, the idea that they are under constant stress does not ordinarily suggest itself. To this habit of superficial observation I ascribe the slowness with which the dynamic element in evolution has received recognition, though pointed out clearly so long ago, by Herbert Spencer.

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*Nature Jan. 9, 1890, p. 227; Science, 1890, p. 110. The name Neo-Lamarckian is objectionable, as it tends to connect with the modern hypothesis the different and obsolete theory of the distinguished French naturalist.

That which distinguishes the organic individual from the inorganic fragment of matter is the complexity of its reäction to these impacts, which reäction we term physiological in contra-distinction to the simply mechanical, though both, at bottom are doubtless similar.

The characters which develop in an organism in response to these impacts are acquired, but that which is transmitted is a facility of response in the same line, which may, under favorable circumstances, lead to a similar response in the progeny, and, in the course of time with a continuation of similar impacts through successive generations, promote and establish the physiological habit which is the directive influence toward the regular development of the characters in question.

It is, I believe, generally admitted that such is the case in relation to mental stimuli and reäctions in man and some of the higher animals and that the growth of intellectual life in the human race depends upon it.

It is a matter of indifference, dynamically, whether the particular series of impacts concerned in developing a special physiological response is the result of conscious effort by the organism or not; but, as it is highly unlikely that any voluntary effort, no matter how seconded by habit, should be as constant and unceasing as the impacts due to ordinary mechanical forces, we should expect the responses due to conscious effort to be feeble in intensity and numerically few in comparison with those arising from the dynamic forces undirected by consciousness.

The dynamics of the environment, so far as we are able to understand them, in their principal features must be remarkably constant. The weight and consistency of the water or air which forms the surrounding medium, the character of the supporting surface, the range of temperature, the supply of light, the friction of adjacent bodies, the attraction of gravitation, vary within comparatively narrow limits, when consistent with organic existence. We should therefore expect that their influence would on the whole be conservative and tend toward the preservation of the main characteristics of organisms once brought into substantial equilibrium with their surroundings.

On the other hand, owing to the very narrowness of the limits within which life is possible, the dynamic variations, within those limits, to which organic forms are subjected become relatively more important. It is probable that since the initiation of life upon the planet no two organisms have ever been subjected to exactly the same dynamic influences during their development. Differences of impact necessarily imply differences in response, hence a certain amount of variation is the inevitable result. It is absolutely impossible that any two individuals can be or ever have been strictly similar and the application of a conception of exact similarity to any two actual beings becomes more and more difficult as the complexity of their organization is increased.

The origin of variation therefore presents no difficulties ;

rather the presence of two strictly similar beings, could it be shown, would border on the miraculous.

The question which demands an answer is, how are the small necessary and admitted differences stimulated to develop into the obvious differences which are recognized by systematic biologists?

To this I would answer that the reäctions of the organism against the physical forces and mechanical properties of its environment are abundantly sufficient, if we are granted a simple organism, with a tendency to grow, to begin with; time for the operation of the forces; and the principle of the survival of the fittest.

It is often assumed in discussing variation that the possibility of variation is equal in every direction. A consideration of the dynamic conditions of life show that this is not the case, or at least, if we grant its theoretic truth, in practice it never can be true. Under any conditions which would permit it, the resulting organic forms would all be sub-spherical, and would have to pass their existence in constant rotation.

The moment that any one of them came to rest it would begin to be subjected to unequal stresses relatively to its different parts. Light, gravity, friction, opportunities for nutrition, would be unequally distributed, with the result of forcing an unequal growth, development, and specialization of its regions. Inequality of form once established, if it were a moving organism, friction and resistence of the circumambient medium would confirm the inequality and put individuals of its kind at a disadvantage when they varied toward the original shape. Flexure of an elongated body would mechanically institute changes analogous to segmentation, as pointed out by Spencer. Any organic mass possessed of mechanical continuity must develop surface tension and initiate a superficial film.

The fact that these portions of matter are organic, in no respect releases them from the common servitude of matter to the laws of mechanics through the operation of physical forces.

If then development of structure is constrained to operate within a limited field, which can hardly be denied, all those calculations based upon the assumption that the field is unlimited fall to the ground and may be safely disregarded as irrelevant.

The operations of biologic selection may be divided into two categories, 1st. those in which fitness and unfitness are determined by the perfection in adjustment of the individual to the mechanics of the environment, which will include the great mass of the lower organisms ; and 2nd., those in which intelligence becomes a factor. The latter will include all forms of sexual selection, mimicry, protective coloration, and every case in which discrimination on the part of pursuer or pursued may come into play. It is by no means necessary that the organism which becomes modified should possess even consciousness, but one of the two parties to the modification, at least, must possess intelligence of a certain grade. The mental qualities of the insect are necessary to the modification of the colors of the orchid, as far as they serve to attract its attention or direct its movements, while the modifications of the stigma or pollen mass to facilitate cross fertilization, fall into the other category.

While the operations of the first category must always have been active, and probably were not supplemented by those of the second category for an immense period of time, yet I believe the latter also to be very ancient. It is probable, however, that influences of the second category operate more rapidly and are productive of much greater diversity in development than could ever have been expected from the unassisted working of the physical forces.

Passing from these general considerations to those of a more special character, the contention of Weismann that "not a single fact hitherto brought forward can be accepted as proof" of the transmission of acquired characters demands attention. This reminds one of the familiar statement of twenty years ago that the Darwinians had not brought forward a single instance of the conversion of one species into another species. If the Dynamic Evolutionist brings forward an hypothesis which explains the facts of nature without violence to sound reasoning, that hypothesis is entitled to respect and consideration until some better one is proposed or some vitiating error is detected in it. No one has yet "proved" that one species is developed out of another species in the sense in which Weismann uses the word proof in his criticism. But plenty of facts which support the hypothesis that acquired characters are transmitted in the sense hereinbefore explained have been accumulated, of which Osborne's paper, above cited, affords evidence in one direction. Can anyone believe that the permanent limb-callosities of the horse and deer, for instance, are selective developments of fortuitous larval corns? Our knowledge of the physiology of any animal, except too or three which have been domesticated for ages, and excepting man, is so contemptibly meagre that it cannot be quoted as evidence on either side.

The question has been much obscured by the attempt to quote the effect or non-effect of mutilations upon progeny, on one side or the other.

For the Dynamic hypothesis only those characters can be considered which arise from permanent physiological reactions due to the impact of external forces. Mutilations rarely fall into this category and are essentially sporadic. In the case of circumcision, so often cited, they affect, at most, half the individuals of a race and only half of any one generation.

There is not a particle of reason to believe that the excision of a trifling scrap of cuticle from an infant would lead to any physiological reäction worthy of attention. One might with greater warrant seek such an effect in the growth of hair and of the nails in civilized races accustomed to trim them. Neither case has been shown to afford valuable evidence.

There is no reason to deny that a pathologic incident of sufficiently fundamental character may effect the progeny of an individual, but it is of no consequence to the Dynamic hypothesis whether it can be proven or not.

Experience shows that it is not single mutilation or loss of substance which results in permanent physiological reäctions so much as continued impacts which lead to locally increased nutrition or local anaemia.

The objection to reasoning drawn from pathologic cases is not that it is not or may not be true, but that the cause affects only individuals in trifling numbers.

The forces invoked by Dynamic hypothesis, on the other hand, affect every individual of a race and every generation as long as the environment continues unchanged. Sporadic modifications must always be finally swallowed up in the general average of the organic type, unless carefully selected by intelligent agencies. The steady pressure of telluric forces lets no individual escape.

On the coast of California the soft tertiary sandstones are drilled by several species of boring mollusks, *Pholas*, *Lithophagus* and *Petricola*. In the course of time the borers die and leave their closely fitting cells untenanted. Into these safe retreats the young of several non-boring bivalves are in the habit of retiring.

As they grow they become too large to escape by the hole through which they entered. Grow they must but the stone walls of their dwelling permit growth only in certain directions. The collector breaks the rock and finds *Kellia*, *Tapes* or *Rupellaria* with the outward conformation of the antecedent borer. Those which refused to conform, if any, have died. Here we have a case where characters have been assumed under an abnormal stress analogous to a pathologic or traumatic mutilation. The progeny of these nestlers would probably exhibit no traces of their parents' deformity. But the pressure of the physical forces on this progeny would be, though invisible, as constant and effective in its results as the rock seemed to be with the nestlers. These results in proportion to their harmony with the environment produce upon the observer the impression which is implied when he speaks of the appearance of such species as "normal."

In my paper on the hinge of Pelecypods and its development,* I have pointed out a number of the particular ways in which the dynamics of the environment may act on the characters of the hinge and shell of bivalve mollusks.

In a paper now in preparation for publication I have shown how the initiation and development of the columellar plaits in *Voluta*, *Mitra* and other Gastropods, is the necessary mechanical result of certain comparatively simple physical conditions; and that the variations and peculiarities connected with these plaits perfectly harmonize with the results which follow with inorganic material subjected to analogous stresses.

Attention once directed to this class of influences and their effects and it is certain facts will accumulate not less numerous and convincing in their establishment of the theory than those which have been taken as "proof" of the survival of the fittest.

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*Am. Journ. of Science, Dec., 1889, p. 445.

NOTE. Since this paper was delivered before the society the discussion of the subject has been continued in the pages of *Nature*. I have been interested to note that Prof. Lankester (in the issue for Mar. 6, 1890, page 414) like the skilled tactician he is, has begun building bridges in his rear which may serve as a means of retreat from his present untenable position. He now explains that by the "transmission of acquired characters" he means the obsolete theory of Lamarck in its purity, which, so far as I have followed the discussion, nobody has proposed to uphold. Why he has continued to oppose the Dynamical theory by arguments intended to demolish a totally different hypothesis, he does not explain.

Mr. Romanes has also pointed out that recent admissions of Dr. Weismann are fatal to the ingenious hypothesis and assumptions with which that gentleman's name has been chiefly connected (*Nature*, Mar. 13, 1890, p. 429.)

In fact these and other signs indicate that the most able of those who have through haste or conservatism been disposed to ignore dynamical influences in evolution, will before long join in the procession, and lend their undoubted abilities to the perfection and elaboration of the only theory yet propounded which fully and efficiently supplements that of Natural Selection and closes the too obvious gaps which have hitherto existed in the intellectual structure of the modern theory of organic evolution.

NEO-DARWINISM AND NEO-LAMARCKISM.*

BY LESTER F. WARD.

INTRODUCTORY.

In casting about for a subject on which to address this Society I have encountered serious difficulties. A presidential address to a Biological Society should, as it seems to me, follow one of two courses. It should either relate in its general aspects to the subject with which its author is most familiar, and so coordinate the facts within his specialty as to correlate them with the sum-total of biological science ; or else, it should be an exposition of and commentary upon the most prominent problem of biology which at the time of its presentation, is engrossing the attention of the scientific world. One year ago I realized these two alternatives as clearly as I do now, and I felt then that while the second of them was not more appropriate than the first, the overwhelming prominence of one great biological question almost demanded that I should sink my individual preferences and, as a matter of sheer duty, undertake to grapple with that question. But I have always believed and often said that the Biological Society should choose as its president one who represents the whole science of biology, and that it made a mistake in selecting a narrow specialist, and a specialist in a department which has the reputation of not keeping pace with the rest of the science, and I felt that if compelled to listen to views growing chiefly out of that narrow specialty, it was in some degree its own fault. But in view of the fact that the Society saw fit to repeat its mistake, and that I last year presented the problems of botany and its geologic history, there seems no escape from that duty which still confronts me of closing with the great problem of heredity which continues to occupy the foreground of all biological discussion.

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* Annual Presidential Address delivered at the Eleventh Anniversary Meeting of the Biological Society of Washington, January 24, 1891, in the Law Lecture Room of the Columbian University.

There is strictly speaking only one prominent question before the biological world and that is the question whether qualities that are acquired after birth are capable of being transmitted to descendants. Darwinism in its original entirety, as expounded by Darwin himself, admits such transmission. But by the new school of Neo-Darwinists this is denied. On the other hand Lamarckism, as expounded by Lamarck, explains all change through the transmission of functionally acquired characters, the law of natural selection not having been perceived by Lamarck. But Neo-Lamarckism, as I understand it, while recognizing natural selection as the more potent of the two agencies, also recognizes that the increments of change impressed upon individuals during their lifetime or brought about by individual efforts or habits are also perpetuated in some measure through heredity and form an important factor in the general process of organic development.

STATUS OF THE PROBLEM.

From the appearance of the Origin of Species in 1859 until within the past four or five years it had been the opinion of nearly all naturalists that the existing forms of animals and plants were the result mainly of two cooperating causes, one of which may be called *functional* and the other *selective*. The multitudinous infinitesimal effects wrought by both of these causes upon the form and character of organisms were believed to be cumulatively perpetuated by heredity in the modification of species and the production of new and altered forms of vegetable and animal life. Prior to the date named the few who conceived that existing forms might be modifications of ancestral ones ascribed the changes wholly to the first of these causes, the functional. Mr. Darwin showed that this could not account for all cases, and in pointing out, simultaneously with Mr. Wallace, the existence and mode of operation of the selective agency he made the most important contribution yet brought forward to the science of biology.

At the date of Darwin's death, 1882, the general doctrine of evolution and the theory of development in biology had been accepted by so nearly the entire body of scientific men that it was scarcely worth the effort to conciliate the small remnant who still adhered to the special creation hypothesis. The only question was : By what agency or agencies is evolution accomplished ?

It would carry me too far to attempt to pass in review the various theories of the pre-Darwinians—Treviranus, De Maillet, Goethe, Buffon, Geoffroy St. Hilaire, Erasmus Darwin, and the anonymous author of the Vestiges of Creation. This task has been admirably performed by Professor Haeckel in his History of Creation, and in the later editions of the Origin of Species Mr. Darwin has collected quite a number of sporadic adumbrations not only of the law of evolution itself but even of that of natural selection. I shall be obliged to confine myself almost exclusively to the one great mind, who far more than all others combined, paved the way for the new science of biology to be founded by Darwin, namely, Lamarck. His life was chiefly devoted to the systematic and structural investigation of animals and plants and his earlier works gave little indication of philosophical tendencies, but his Philosophie Zoologique,* which appeared in 1809, showed that he had reasoned deeply about the objects he had so long studied, and in this work is contained the whole of his celebrated system of the transmutation of species.

LAMARCKISM.

Although most of the members of this Society are doubtless familiar with the general character of the Lamarckian philosophy, and many have probably read this work, the nature of my subject seems to demand that some of the more general principles of Lamarckism be set forth. A few paragraphs from the work in question will accomplish this better than any attempt at exposition. The following quotations will serve to show the sweeping character of Lamarck's generalizations, and when we remember the time at which they were written it will not appear strange that his views attracted so few adherents and had to wait half a century for a respectful hearing.

"In order to judge" says Lamarck "whether the idea that has been formed of a *species* has any real foundation, let us return to the considerations which I have already set forth; they show:

1st. That all the organized bodies of our globe are true products of Nature, which she has successfully brought forth (*exécutés*) in the course of long periods of time;

2d. That in her march Nature has commenced, and is still daily commencing to form the most simple organized bodies, and that she only forms these latter directly, that is, only these first sketches of organization that have been designated by the expression *spontaneous generation;*

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* Philosophie Zoologique, etc. Par Jean-Baptiste-Pierre-Antoine de Monet, Chevalier de Lamarck. Nouvelle Edition, revue et précédée d'une introduction biographique par Charles Martins. Paris, 1873, 2 vols. 8°.

3d. That these first outlines of the animal and of the plant formed at suitable places and under appropriate circumstances, and possessing the attributes of incipient life and organic movement, have themselves little by little developed organs and in time multiplied these as well as parts;

4th. That the power of growth in each part of the organized body being inherent in the first effects of life, it has given rise to the different modes of multiplication and reproduction of individuals, and that in this way the progress acquired in organization and in the form and diversity of parts has been preserved;

5th. That by the aid of sufficient time, of circumstances which have been necessarily favorable, of the changes which all points on the surface of the globe have successively undergone in their condition, in a word, by the power which new situations and new habits possess to modify the organs of bodies and of life, all the organisms that now exist have been insensibly formed such as we see them;

6th. Finally, that under the influence of such an order of things, living bodies having undergone each of the changes, greater or less, in their structure and in their parts, what is called *species* thus insensibly and successively brought about among them has only a relative constancy in its character and cannot be as ancient as Nature herself." *

It will be seen that both the mutability and the transmutation of species are distinctly formulated. But in order to make this more clear he elsewhere says:

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* Op. cit., Vol. I, pp. 81-83.

"In the same climate very different situations and exposures cause individuals thus placed at first simply to vary; but in the course of time the continual differences of situation of the individuals of which I speak, living and reproducing under the same conditions, bring about in them differences which become in some sort essential to their existence, so that at the end of many generations which succeed each other those individuals which belonged originally to another species find themselves transformed into a new species, distinct from the other." *

The two great causes to which he ascribes this transformation are: 1, what he calls the "circumstances," and 2, the "habits" of the creatures transformed, and to enforce this idea he lays down the following "zoological principle" the fundamental truth of which, he says, appeared to him incontestable:

"Progress in the structure (*composition*) of the organization undergoes here and there in the general series of animals anomalies brought about by the character of the habitat (*circonstances d'habitation*), and by that of habits contracted."†

Between the "circumstances" and the "habits," however, Lamarck perceived a casual relation, and this he expressed in the following argumentative form:

"The true order of things to be considered in all this consists in recognizing:

1st. That every permanent change of any consequence in the circumstances under which each race of animals finds itself effects within it a real change in their needs;

2d. That every change in the needs of animals necessitates for them other actions to satisfy the new needs, and hence, other habits;

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* Op. cit., Vol. I, pp. 79-80.

† Op. cit., Vol. I, p. 145.

3d. That every new need necessitating new actions to satisfy it requires of the animal experiencing it either the more frequent employment of such of its organs of which it previously made less use, which develops and enlarges considerably, or else the employment of new parts to which the needs insensibly give rise within it through efforts of its internal sense."*

In all this Lamarck does not expressly say that these transformations are perpetuated by heredity, although this is clearly implied, otherwise they would not be permanent. But he now proceeds to embody the general principle in what he calls a *law*, the first law of organic life, to which he adds a second law in which the principle of heredity is distinctly formulated. Although these two great Lamarckian laws have been frequently quoted in biological discussions, especially within the past three or four years, it seems essential to the completeness of the present exposition to introduce them. They are as follows:

First Law: "In every animal which has not passed the limit of its development the more frequent and sustained exercise (*emploi*) of any organ gradually strengthens that organ, develops and enlarges it, and gives it a power proportioned to the duration of such exercise; while the continued lack of exercise (*usage*) of an organ gradually enfeebles it, deteriorates it, progressively diminishes its power, and finally causes it to disappear."

Second Law: "All that nature has caused individuals to acquire or to lose through the influence of the circumstances to which their race has found itself for a long time exposed, and consequently, through the predominant exercise of certain organs, or through a failure to exercise certain parts, it preserves through heredity (*génération*) to the new individuals that are produced by them, provided the changes acquired are common to the two sexes, or to those that have produced these new individuals." *

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* Op. cit., Vol. I, pp. 234-235.

These laws are enforced by considerable iteration and all the facts and illustrations that he could command. He condenses his first law into the following form :

"The frequent exercise of an organ which through habit has become permanent increases the capacity of such organ, develops it, and causes it to acquire dimensions and power of action which it does not possess in animals that exercise it less." †

The second law is re-stated in the following language :

"Every change acquired in an organ by an habitual exercise sufficient to have brought it about, is preserved thereafter through heredity (*génération*) if it is common to the individuals which, in fecundation, unite in the reproduction of their species." ‡

Such is Lamarckism pure and simple, which it seems necessary to set forth at first hand before approaching those modern phases of the problem which have grown out of it. It is obvious that it deals only with the first of the two agencies in biologic progress mentioned at the outset, namely the functional ; and Lamarck, although he clearly grasped the law of competition, or the struggle for existence, the law of adaptation, or the correspondence of the organism to the changing environment, the transmutation of species, and the genealogical descent of all organic beings, the more complex from the more simple ; he nevertheless failed to conceive the selective principle as formulated by Darwin and Wallace, which so admirably complemented these great laws.

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* Op. cit., Vol. I, pp. 235–236.

† Op. cit., Vol. I, p. 247.

‡ Op. cit., Vol. I, pp. 258–259.

The cogency of Lamarck's reasoning, especially when we consider the time at which he wrote, is sufficiently apparent to all, but it may not be without interest to note the manner in which it struck so excellent a judge as Professor Huxley as late as 1876. In contrasting it with the views of Cuvier who maintained the fixity of species and their special creation, Professor Huxley says: "It is impossible to read the 'Discours sur les Revolutions' of Cuvier, and the 'Principes' of Lamarck without being struck with the superiority of the former in sobriety of thought, precision of statement, and coolness of judgment. But it is no less impossible to consider the present state of biological science without being impressed by the circumstance that it is the conception of Lamarck which has triumphed and that of Cuvier which has been vanquished . . . It is not too much to say that the facts of biology known at the present day are all consistent with and in favor of the view of species entertained by Lamarck, while they are unfavorable to, if not incompatible with, that advocated by Cuvier."*

DARWINISM.

Darwin was acquainted with Lamarck's views when he wrote the Origin of Species, and notwithstanding the fact that whenever he refers to Lamarck, as he does in several of his letters† he does so in a very disparaging way, he must have been greatly influenced by them, or at least by views of the same import expressed by others as well as by Lamarck, but especially those of his grandfather Erasmus Darwin, who anticipated, rather from the standpoint of the poet and seer, the truths to which Lamarck was led by a life-long study of living things.

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* Am. Cycl., Art. Species.

† Life and Letters, Vol. I, p. 542, Vol. II, p. 198.

But Darwin, like most other thorough naturalists, was little satisfied with the Lamarckian theory, because it left, as all now admit, so much still unexplained, but instead of rejecting it in toto, as most other naturalists did, he sought, and happily succeeded in finding the principle on which the remainder of the facts could be accounted for; or, at least, the greater part of them, for it seems that however deeply we may probe the secrets of nature there will ever remain a few residual phenomena that refuse to submit to our canons.

It is certainly unnecessary that I should occupy your time with any extended exposition of the law of selection, and I will content myself with the following bare definitions:

Natural selection is the general law that variations are constantly occurring in organized beings, and that such of these variations as prove advantageous to the species are preserved through heredity and transmitted to posterity while those which are not advantageous or are disadvantageous to the species are not preserved nor transmitted; the cause of such selection being the fact that advantageous variations tend to increase the chances that the individuals possessing them will reach the reproductive age and continue longer to reproduce, and will hence leave a larger number of offspring than those individuals which had not varied or had varied in an equivocal or unfavorable manner.

Artificial selection is the act of man in intelligently selecting the individuals that possess in the highest degree the particular qualities that he desires to produce as the parents of the animals or plants which he wishes to domesticate or cultivate. The eminent success obtained by man in this way is the certain proof that the qualities of the parents are transmitted to their offspring, and explains the efficacy of natural selection.

Sexual selection is the law that one sex, usually the female, exercises a choice between the individuals of the other, whereby those individuals possessing the selected qualities stand a much greater chance to have the opportunity of transmitting them to their offspring. This law explains the greater ornamentation of the males in so many species, since most such characters are peculiar to one sex and only appear at maturity. Sexual selection also checks the tendency of natural selection to extreme variation in certain directions, since the sexes are well known to prefer their opposites, which causes the offspring to occupy a mean between the extremes. This effect is very marked in the human race, but is doubtless operative among the lower animals. As I pointed out in last year's address, sexual selection has wrought a great revolution in the relative size, strength, and beauty of the two sexes, and reversed in birds and mammals the normal law of female superiority which prevails in most of the lower departments of life.

ACQUIRED CHARACTERS.

It will be readily perceived from what has been said of the two great principles of transformism, the functional, as set forth by Lamarck, and the selective, as elaborated by Darwin, that the fundamental distinction between them is that in the former the transforming qualities which are to be cumulatively transmitted through heredity to the descendants of a given ancestral pair are acquired during the lifetime of these individuals, whereas in the latter the transforming increment is a merely accidental modification arising from unknown causes and hence called spontaneous. The theory is that such spontaneous variations are constantly taking place in all individuals, some in one direction and some in another, and that all except the advantageous ones are immediately lost, while such as tend to increase the chances of survival in the struggle for existence are preserved. Nature has thus provided, through this survival of the fittest, for the maintenance of the equilibrium between the organism and the environment, and also for the increase of structural adaptation and vital power, independently both of the effort of the individual to conform more exactly to its surroundings and of the reaction of the organism upon the impinging environment.

There has never been any doubt of the perfect transmissibility of these spontaneous modifications, or, as they have been called, fortuitous variations. They belong to the essential nature of the organism, and have, as we shall see later on, been ingeniously explained as originating in the very germ itself.

But with regard to functional modifications, or as they are more commonly called, acquired characters, grave doubts have arisen in the minds of many naturalists as to whether they are capable of being inherited by the descendants of those in which they have been superinduced. They are in a certain sense foreign to the organism, external and superficial, and the great question has been how they can succeed in so affecting the reproductive germs of the parents as to reappear in the offspring. That Darwin believed in the transmission of functionally acquired characters is attested not only by many passages in which this belief is expressly stated but by the bringing together by him of more facts in support of it than have been given by all other writers combined either before or since. And although the greater part of his work was naturally directed to the establishment of the hitherto unknown, but as he believed, more important law of selection, nevertheless Darwinism proper must be made broad enough to embrace both of the great agencies of organic transformation, the functional and the selective.

It is hardly necessary to add that pure Lamarckism has nothing whatever to do with such a question as whether accidental modifications produced upon an organism, such as mutilations from whatever cause, are inheritable, since these are not due to continuous interaction between organism and environment, are not the objects of the creature's efforts, and are not acquired by any functional or habitual activities. And yet it is no exaggeration to say that at least one-half, probably much more, of the space devoted by the Neo-Darwinians to the supposed refutation of Lamarckism has been directed to proving that acccidental mutilations are not transmitted to offspring.

I do not deny that there is a doctrine of the transmissibility of mutilations, and Darwin and others have collected a large body of facts pointing strongly in that direction, while Brown-Séquard is believed by many to have demonstrated that hereditary epilepsy may be artificially superinduced in guinea-pigs by lesions of the brain. And it may be that Lamarck, coming upon similar facts, gave them a certain credence, but, as we have seen from typical passages quoted from his work, these cases are not capable of being used in support of his general philosophy, which rests entirely upon the effects of functional activities exerted in response to secular alterations in the surrounding conditions of existence.

Whatever of truth, therefore, there may be in the doctrine of the transmissibility of suddenly or accidentally acquired characters, it is clearly outside the present discussion and need not be further touched upon.

After the doctrine of natural selection had been clearly explained it was found to be so simple and at the same time so far-reaching that it began to be questioned whether much that had been formerly attributed to the other agency ought not to be credited to it instead ; and it cannot be denied that this inquiry tended to broaden the field of the selective at the expense of that of the functional principle. So clear and certain are the workings of the former that it is considered safe to credit it with every fact that can be explained by it, even though it be also explicable by the other law.

But it was not allowed to rest here. The difficulties in the way of accounting for the transmission of qualities originating after the birth of the parents appeared to some so great that they began to doubt whether in fact such a thing is really possible. Of course, there were many popular and superficial writers on evolution who fail to distinguish the two principles and talked as though all development was due to natural selection, so that to the unscientific and popular mind evolution and natural selection were largely synonymous and vaguely comprehended, as is, in fact, still to a large extent, the case. Other better informed people, including some naturalists of note, were so dazzled by the new idea that they lost sight of the old one, and habitually ignored the functional element without criticising it or taking any account of it. It appears to have been against this class that Mr. Herbert Spencer's brilliant exposition of the principle which, in characteristic language, he calls "direct equilibration" was directed. To this I shall have occasion to revert.

For the present I propose to confine myself to those writers who clearly comprehend the nature of the two principles, and who either gravely doubt for what seem to them sufficient reasons, or else deny altogether the efficacy of functional modification and the doctrine of the transmission of acquired characters. The limits of an address such as this preclude any effort to make the discussion historically exhaustive by enumerating all the investigators who from the first to last have taken this view, or some modified form of it, and I shall be content to name among Germans Du Boise Reymond, Pflüger, His, and Weismann, and among Englishmen Galton, Wallace, and Ray Lankester; while what I shall have time to say relative to the nature of the objections raised by these authors will be chiefly confined, for the present, to the views of Galton and Weismann.

THEORIES OF HEREDITY.

It must, however, be premised that inasmuch as the objections raised against the doctrine of the transmission of acquired characters are based upon the difficulties encountered in attempting to explain how such characters can impress themselves upon the germ, all those who have doubted or denied such transmission have approached the subject from the side of embryology, which makes their arguments difficult to explain to biologists in general and still more so to the general public. The laws and processes of heredity are still in the stage of mystery, and their mysterious character has led to many erroneous beliefs and popular superstitions. It is a significant fact that all the mysteries that have been thus far cleared up by science—astronomical, physical and chemical mysteries—have been shown to be the expressions of previously unknown laws of matter and force, and to rest upon a purely material and mechanical basis. The chief obstacle to their comprehension has been the minuteness of the material elements in action—a minuteness far beyond the capacity of the most powerful artificial aids to the senses—so that their secrets have had to be wrung from them by ingenious and multiplied experiments upon their effects. Now, the ultimate reproductive elements, though doubtless many times larger than any chemical molecule, even the most complex, such as those of protein and other organic compounds, are doubtless still far too minute to be observed by the highest powers of the microscope, and if the entire history of the formation of a new organic being is ever to be learned it must be by a successful study of the actions of such minute objects. But this is infinitely more difficult than the study of the actions of inorganic elements, since they take place within an organism whose destruction destroys their vital character.

In view of the history of the less complex sciences it is natural that biologists should insist that the phenomena of heredity are due to the activities of the ultimate material reproductive elements, and not to any vague and occult force or *deus ex machina.* Consequently we find that the only theories of heredity that have been put forth have been based on this assumption.

One of the earliest, and certainly the most celebrated of such theories is Darwin's *pangenesis*, published in 1867.* According to this theory, which is doubtless familiar to most of you, the ultimate reproductive elements, called *gemmules*, are given off from the cells of all parts of the body and collect in the germ-cells and sperm-cells, so that the fertilized ova contain literal representatives of every organ and every part of both parents, which in the new being return to their respective locations and cause the repetition in each of the exact qualities possessed by the parental organs or parts, subject, of course, to the modifications due to a conflict or cooperation between the gemmules of the two parents, equalizing a character where they are different, and emphasizing it where they are alike.

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*Variations of Animals and Plants under Domestication. Vol. II, Chaps. XXXVII, XXXVIII.

It will be readily seen that this theory adapts itself to the broadest conception of heredity and, if true, accounts for the transmission of functionally produced modifications as well as the selection of such accidental ones as prove advantageous. But to the ordinary mind this strictly materialistic explanation of heredity seems crude and is to a large extent unintelligible, and the doctrine of pangenesis has gained few adherents among scientific men. They fail as a rule to comprehend Mr. Darwin's gemmules and to understand how they should behave in the manner required by the theory.

Very much of this difficulty, however, is cleared away by the admirable exposition of Mr. Herbert Spencer of the nature of what he calls physiological units. To the biologist the organic unit is the cell and when he has explained the nature and action of cells he thinks he has gone far enough. But the facts of heredity cannot be explained by any phenomena manifested by cells. Between the cell or morphological unit in biology and the molecule of a highly complex organic compound, such as albumen,—the highest class of chemical units—no intermediate element had hitherto been recognized. Mr. Darwin's gemmule is clearly such an intermediate element, and the question at once arose, is there any such? Mr. Spencer has, I think, shown beyond the possibility of doubt that there is and must necessarily be such an element—a unit which is not chemical, since it possesses life, and which is not the morphological unit or cell, but is that of which the active part of every cell consists, and is appropriately termed the physiological unit. I have elsewhere* undertaken to show that life may have resulted from a process of chemical recompounding, and may actually constitute the leading property of the highest organic compound protoplasm, and I venture to suggest here that the gemmules of Darwin and the physiological units of Spencer may be nothing more than the molecules of protoplasm, which, as I have explained, are so immensely complex that any required degree of difference in their essential constitution may easily exist.

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*American Naturalist, Vol. XVI, December, 1882, p. 976. Dynamic Sociology, New York, 1883, Vol. I, p. 311.

The only other theory of heredity which time will warrant my mentioning now is that of Professor Haeckel, published in 1876 and known as "the perigenesis of the plastidule." To avoid the possibility of misstatement, I will give this theory in the words of its author, as epitomized in the latest (8th) edition (1889) of his Schöpfungsgeschichte (pp. 200–201): "The perigenesis-theory was founded by me in 1876 in a memoir 'on the wave-reproduction of vital particles or the perigenesis of plastidules,' and as a 'provisional attempt at a mechanical explanation of the elementary processes of development,' and especially of heredity. (In the second part of my collection of popular lectures, Bonn, 1879, pp. 25–80). The perigenesis-hypothesis seeks to explain heredity by a simple mechanical principle, namely, by the well-known principle of transmitted motion. I assume that in every process of reproduction not only is the special chemical composition of the plasson or plasma transmitted from the parent to the offspring, but also the special form of molecular motion which belongs to its physico-chemical nature. In harmony with the fundamental laws of modern histology and histogeny, I assume that this plasma (either the caryo-plasma of the cell-nucleus or the cytoplasma of the cell-body) is alone the original bearer of all vital activity, and hence also of heredity and reproduction. In all plastids (as well the anucleated cytodes as the genuine nucleated cells) this plasma or plasson is composed of plastidules or plasma-molecules, and these are 'probably surrounded by aqueous envelops; the greater or less thickness of these aqueous envelops, which at once separate and bind the neighboring plastidules, determines the softer or harder condition of the flowing plasson' (p. 48).

'Heredity is the transmission of plastidule motion, whereas adaptation is change of plastidule motion' (p. 55). This motion may in its general aspects be conceived as a ramified wave-motion. In all protists or unicellular organisms (protophytes and protozoans) this periodical movement of the mass goes on in a correspondingly simple manner while in all tissue-bearing or multicellular creatures (metaphytes and metazoans) it is combined with a mutual generation of the plastids and a division of labor of the plastidules.''

It will be observed that although this theory of heredity lays special stress upon the idea of motion, thereby recognizing the element of force, it is nevertheless based like all others upon the existence of ultimate material elements different on the one hand from the chemical molecules and on the other from cells, and intermediate between these. The gemmule of Darwin, the physiological unit of Spencer, and the plastidule of Haeckel are the same in essence, and the study of the phenomena of these ultimate elements of biology open up a new and most promising field of research into which scarcely any investigator has as yet deliberately entered.

We are now prepared to consider the objections of Galton and Weismann to the doctrine of the transmission of functionally acquired characters.

VIEWS OF MR. GALTON.

The earliest expression of Mr. Galton's views, so far as I am aware, is contained in a paper ''On Blood-Relationship'' presented by him on June 13, 1872, to the Royal Society of London and published in its proceedings.* In this paper stress is laid upon the distinction in embryonic development between what he calls the "patent" and the "latent" elements, and he argues from the facts of reversion and atavism that the greater part of the parental elements in heredity are latent in the germ, but prepared to express themselves in more or less remote decendants. Although he addresses himself to the anthropologist rather than the biologist, and claims only to be making a contribution to the difficult subject of kinship, he nevertheless evinces a clear grasp of the embryonic conditions of the problem, and as we shall see, anticipates, some of the more exact conceptions of Weismann. He does not wholly deny the possibility of the transmission of acquired characters, but says that "the effects of use and disuse of limbs, and those of habit, are transmitted to posterity in only a very slight degree."

In this respect Mr. Galton makes only a slight advance toward the conclusions of Weismann in the much more elaborate paper which he read before the Anthropological Institute of Great Britain on November 9, 1875, and which appeared in the December number or the Contemporary Review for that year, and also in an expanded form in the Journal of the Institute (Vol. V, p. 329). In this paper which is entitled "A Theory of Heredity," he, however, approaches the main question with much greater directness. "The facts" he says "for which a complete theory of heredity must account may conveniently be divided into two groups ; the one refers to those inborn or congenital peculiarities that were also congenital in one or more ancestors, the other to those that were not congenital in the ancestors, but were acquired for the first time by one or more of them during their lifetime, owing to some change in the condition of their life. The first of these two groups is of predominant importance, in respect to the number of well-ascertained facts that it contains, many of which it is possible to explain in a broad and general way, by more than one theory based on the hypothesis of organic units. The second group includes much of which the evidence is questionable or difficult of verification and which, as I shall endeavor to show, does not for the most part, justify the conclusion commonly derived from it.'' He further says that his theory ''is largely based on the arguments and conditions brought forward by Mr. Darwin in support of pangenesis; nevertheless the conclusions in this paper will be seen to differ essentially from his own. Pangenesis appears more especially framed to account for the cases which fall in the second of the above-mentioned groups which are of a less striking and assured character than those of the first group, and it will be seen that I accept the theory of pangenesis with considerable modification, as a supplementary and subordinate part of a complete theory of heredity, but by no means for the primary and more important part.''

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*Vol. XX, p. 394.

He employs the term *stirp* ''in a special sense—to express the sum-total of the germs, gemmules, or whatever they may be called, which are to be found, according to every theory of organic units, in the newly fertilized ovum—that is in its earliest pre-embryonic stage.'' In defending the theory of organic units he says: ''We must bear in mind that the alternative hypothesis of a general plastic force resembles that of other mystic conceptions current in the early stages of many branches of physical science, all of which yielded to molecular views, as knowledge increased.''

The paper is an exceedingly luminous contribution to the subject, and the theory advanced may be designated in general terms as the doctrine of natural selection or survival of the fittest among the organic units constituting the stirp, to determine which shall become manifest in the offspring and which shall lie latent to reappear or not in later generations. As the stirp contains organic units that have lain latent in previous generations and may become patent in the generation in question, the theory accounts for reversion, atavism, and the whole train of facts in heredity that have so long puzzled the scientific investigator. We are at present only concerned with so much of it as relates to the transmission of acquired characters. The following passage expresses his views on this point: "We have thus far dealt with three agents—(1) the stirp, which is an organized aggregate of a host of germs; (2) the personal structure, developed out of a small portion of these germs; and (3) the sexual elements, generated by the residuum of the stirp. The cases before us are those which are supposed to prove that 2 reacts on 3—that is, the personal structure upon the sexual elements. The first and largest class of these cases refer to adaptivity of race. It is said that the structure of an animal changes when he is placed under changed conditions; that his offspring inherit some of his change; and that they vary still further on their own account, in the same direction, and so on through successive generations, until a notable change in the congenital characteristics of the race has been effected. Hence it is concluded that a change in the personal structure has reacted on the sexual elements. For my part, I object to so general a conclusion." And he proceeds to elaborate his reasons for such objection. Passing over these for want of time I will conclude this exposition of Galton's views by quoting the following passage:

"The conclusion to be drawn from the foregoing arguments is, that we might almost reserve our belief that the structural cells can react on the sexual elements at all, and we may be confident that at the most they do so in a very faint degree; in other words that acquired modifications are barely, if at all, *inherited*, in the correct sense of that word. If they were not heritable, then the second group of cases would vanish, and we should be absolved from all further trouble about them; but if they exist, in however faint a degree, a complete theory of heredity must account for them. I propose, as already stated, to accept the supposition of their being faintly heritable, and to account for them by a modification of pangenesis."

I am not aware that Mr. Galton has modified the views here expressed since the date of that paper, but in all his subsequent ones, as well as in his work on "Hereditary Genius" (1879) he continues to emphasize the paramount importance of the latent elements in heredity, and the superiority, as he forcibly expresses it, of nature over nurture.

TEACHINGS OF PROFESSOR WEISMANN.

The vigorous onslaught which has been made upon the doctrine of the transmission of acquired characters, since the date of Mr. Galton's papers, and apparently without a knowledge of them, by Prof. August Weismann of the University of Freiburg has probably aroused a greater amount of interest among scientific men than any other event that has transpired since the appearance of Darwin's *Origin of Species*. Professor Weismann is an embryologist and histologist and has conducted a series of prolonged and successful investigations upon several groups of lowly organisms. But he has looked beyond the special facts which are immediately connected with his researches and has thought out for himself all the deeper problems of biology. Besides making himself complete master of the whole field of that science as generally accepted he has coordinated its facts and drawn from them a number of new and brilliant conclusions which have set the world to work on entirely new lines of investigation.

Professor Weismann was logically led to the conclusion that acquired characters cannot in any conceivable way be transmitted. The first of the series of essays which have produced such a sensation, that on the duration of life, was originally read before the Association of German Naturalists and Physicians at Salzburg in September, 1881, and a short abstract of it appeared in *Nature* for April 5, 1888 (Vol. XXXVII, p. 541). It was in this paper that he elaborated the theory that unicellular organisms are potentially immortal. The second of the series, that on heredity, was his inaugural address as Pro-rector of the University of Freiburg, delivered June 21, 1883. It was in this that he first attacked the doctrine of the transmission of acquired characters, and in it and the preceding essay may be found the germs of all his later theories. The remaining six essays appeared at intervals from 1883 to 1888. Abstracts and reviews of them occured in *Nature* and the English magazines, and long before the appearance in 1889 of the admirable work containing an English translation of the whole series with numerous additions and amendments by the author and notes by the translators,* the controversy had begun in which so many of the most eminent biologists of Europe and America have taken part.

Professor Weismann's general course of reasoning is somewhat as follows: It is universally admitted that all the higher organisms consist of tissues made up of cells and that these cells do not differ essentially from those which are found leading an independent existence and are termed unicellular organisms. Many of these unicellular organisms reproduce by the process known as fission or division ; that is, they split or divide into two equal parts each of which becomes a new organism exactly like the original. These halves exist for an appointed time, increase in size until they are each equal to the original cell before division, and then divide again, so that what was formerly one now becomes four. Each of these four repeats the process, and so on, thus multiplying in a geometrical ratio. But if we follow any one of these lines of descent we observe that the last of the line contains some of the same matter that was in the first, and none of the matter has ceased to live. Unless destroyed by some external cause all of the substance of the original cell will continue to live for any conceivable length of time. It is "potentially immortal." Now, the theory of descent as a universal organic principle, which Weismann fully accepts, explains all the life of the globe as resulting from previous life through some form of reproduction. Fission is the simplest form of reproduction, and it is found that it is the common form of cell-reproduction within the tissues of the higher animals. All growth is brought about by it or some modification of it. A study of the phenomena of reproduction in the lower organisms shows that it takes place ultimately through some similar process, which, however greatly modified in its details, consists essentially in the actual transmission of the reproductive cell-substance from parent to offspring, and Weismann maintains that the reproductive cells, like those of unicellular organisms, are immortal or perpetual, and that nothing can get into the body of the offspring except through that of one or other of its parents. This is his fundamental doctrine of the continuity of the germ-plasm. The impregnated ovum contains the germ-plasms of the two parents, and out of it the embryo is formed. The embryo develops independently of the mother by a circulation of its own, and no external influences can by any conceivable method affect or change the characters of the offspring.

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* Essays upon Heredity and kindred biological problems. By August Weismann. Authorized translation edited by Edward B. Poulton, Selmar Schönland, and Arthur E. Shipley. Oxford, Clarendon Press, 1889.

But it is well known that variation takes place, that the offspring does not always resemble either parent, and that changes go on so great as to result in the creation of new species, new genera, and entirely new types of life. All this Weismann admits. How does he explain it? Primarily by natural selection, but he does not stop with that. It has always been admitted that natural selection did not explain the cause of variation. Weismann attempts to do this, and his reasoning is exceedingly ingenious.

The original reproductive cells are assumed by him to consist of an indefinite number of units which he calls germ-plasms, and their presence is explained on the assumption of their preservation from ancestral organisms. Asexual reproduction is of course incapable of producing variation, and he maintains that sexual reproduction has been developed and exists solely for the purpose of insuring variation.

Relative to the constitution of the germ-plasm he says: "Every detail in the whole organism must be represented in the germ-plasm by its own special and peculiar arrangement of the groups of molecules (micellæ of Nägeli) and the germ-plasm not only contains the whole of the quantitative and qualitative characters of the species, but also all individual variations as far as these are hereditary: for example the small depression in the center of the chin noticed in some families. The physical causes of all apparently unimportant hereditary habits or structures, of hereditary talents, and other mental peculiarities, must all be contained in the minute quantity of germ-plasm which is possessed by the nucleus of a germ-cell ; not indeed, as the preformed germs of structure (the gemmules of pangenesis), but as variations in its molecular constitution ; if this be impossible, such characters could not be inherited" (pp. 100, 101).

The union of two germ-cells from entirely different individuals always multiplies the number of ancestral germ-plasms by two. The excess is kept down by the removal of the second polar body, as he supposed was proved by its not taking place in parthenogenesis. But the part removed as well as the part retained contains germ-plasms from both parents alike and hence the offspring must partake of the nature of both.

These ancestral germ-plasms exist in the reproductive cells in vast numbers, and in the removal of half of them at each union of the sexes, there must remain not merely those of the immediate parents, but those of previous generations. If we were theoretically to conceive that at the outset only a single germ-plasm existed from each parent, then the second generation would transmit four, the third eight, and so on in a geometrical ratio, until they would become so numerous as to require the removal of a portion and ultimately always of half the ancestral germ-plasms at each act of reproduction. Says Professor Weismann : "These different qualities are what I have called the ancestral germ-plasms, i. e., the germ-plasms of the different ancestors, which must be contained in vast numbers, but in a very minute quantities, in the nuclear thread. The supposition of a vast number is not only required by the phenomena of heredity but also results from the comparatively great length of the nuclear thread ; furthermore it implies that each of them is present in very small quantity. The vast number together with the minute quantity of the ancestral germ-plasms permit us to conclude that they are, upon the whole, arranged in a linear manner in the thin thread-like loops; in fact the longitudinal splitting of these loops appears to me to be almost a proof of the existence of such an arrangement, for without this supposition the process would cease to have any meaning" (pp. 359–360).

His general view of the origin of variation is thus given by him: "It is well known that this process [sexual or amphigonic reproduction] consists in the coalescence of two distinct germ-cells, or perhaps only of their nuclei. These germ-cells contain the germ-substance, the germ-plasm, and this again, owing to its specific molecular structure, is the bearer of the hereditary tendencies of the organism from which the germ-cell has been derived. Thus in amphigonic reproduction two groups of hereditary tendencies are as it were combined. I regard this combination as the cause of hereditary individual characters, and I believe that the production of such characters is the true significance of amphigonic reproduction. The object of this process is to create those individual differences which form the material out of which natural selection produces new species" (p. 272). "I do not know what meaning can be attributed to sexual reproduction other than the creation of hereditary individual characters to form the material upon which natural selection may work" (p. 281). "The most important duty of sexual reproduction is to preserve and continually call forth individual variability, the foundation upon which the transformation of species is built" (p. 373). "Sexual reproduction is to be explained as an arrangement which ensures an ever-varying supply of individual differences" (p. 384).

Weismann's classification of cells into somatic and reproductive is fundamental to his whole philosophy. On this point he says: "The first multicellular organism was probably a cluster of similar cells, but these units soon lost their original homogeneity. As the result of mere relative position, some of the cells were especially fitted to provide for the nutrition of the colony, while others undertook the work of reproduction. Hence the single group would come to be divided into two groups of cells, which may be called somatic and reproductive—the cells of the body as opposed to those which are concerned with reproduction (p. 27) . . . As the complexity of the metazoan body increased, the two groups of cells became more sharply separated from each other. Very soon the somatic cells surpassed the reproductive in number, and during the increase they became more and more broken up by the principle of the division of labor into sharply separated systems of tissues. As these changes took place the power of reproducing large parts of the organism was lost, while the power of reproducing the whole individual became concentrated in the reproductive cells alone'' (p. 28). His theory further assumes that the germ-cells contain two kinds of plasm, which he calls respectively the *ovogenetic* and the *somatogenic*, i. e., the first capable only of producing germ-cells, the latter capable only of producing somatic cells. These exist together in the fertilized ovum, and if allowed to remain there would go on reproducing themselves in something like equal numbers. But the body consisting almost entirely of somatic cells, it is evident that such a multiplication of germ-cells would be only a hindrance to development. This, he claims, explains the mysterious phenomena so long observed by embryologists and called the removal of polar bodies. The polar body first removed is nothing more nor less than the ovogenetic nucleo-plasm, which is now in the way, and whose removal is necessary to the formation of the embryo. This is the work alone of the somatic cells, and these, consisting as they do of the germ-plasms of an indefinite series of ancestors, and containing representatives of every part of the parent organism, proceed to reproduce a new creature on the hereditary type of the parents with the modifications due to the commingling of many ancestral types.

Without dwelling longer upon these ultimate processes which constitute the premises of Weismann's argument, I will now proceed to state his conclusion. It is simply that he is utterly unable to see how the somatic cells of an adult individual can react upon or in any way affect its reproductive cells. If it cannot, the transmission through either parent to its offspring of any peculiarity acquired since the embryo of the parent began to form is impossible. Firmly believing in the truth of his theory he stoutly insists that no such thing can take place. Of course it needs to be clearly understood what he means by acquired characters, and here, it is claimed lies the chief point in dispute between the Neo-Darwinians and the Neo-Lamarckians. The former contend that the latter class as acquired characters those which are simply due to natural selection. It will therefore be profitable to dwell a moment upon this point.

"The tendencies of heredity", says Weismann, "of which the germ-plasm is the bearer, depend upon this very molecular structure, and hence only those characters can be transmitted through successive generations which have been previously inherited, viz., those characters which were potentially contained in the structure of the germ-plasm. It also follows that those other characters which have been acquired by the influence of special external conditions, during the lifetime of the parent, cannot be transmitted at all" (p. 267). "It is only by supposing that these changes arose from molecular alterations in the reproductive cell that we can understand how the reproductive cells of the next generation can originate the same changes in the cells which are developed from them ; and it is impossible to imagine any way in which the transmission of changes produced by the direct action of external forces upon the somatic cells, can be brought about . . . To this class of phenomena of course belong those acts of will which call forth the functional activity of certain groups of cells" (p. 80). "Only those new characters can be called 'acquired' which owe their origin to external influences, and the term 'acquired' must be denied to those which wholly depend upon the mysterious relationship between the different hereditary tendencies which meet in the fertilized ovum. These latter are not 'acquired' but inherited, although the ancestors did not possess them as such, but only, as it were, the elements of which they are composed" (p. 252). "If acquired characters are brought forward in connexion with the question of the transformation of species, the term 'acquired' must only be applied to those characters which do not arise from within the organism, but which arise as the reaction of the organism under some external stimulus, most commonly as the consequence of the increased or diminished use of an organ or part" (p. 322).

That such characters cannot be inherited he asserts with the strongest emphasis and frequent iteration. His treatment of this point often borders on the dogmatic, as a few extracts will show.

"It has never been proved" he says, "that acquired characters are transmitted, and it has never been demonstrated, that, without the aid of such transmission, the evolution of the organic world becomes unintelligible. The inheritance of acquired characters has never been proved, either by means of direct observation or by experiment" (p. 81). "No single fact is known that really proves that acquired characters can be transmitted" (p. 267). "If acquired characters cannot be transmitted the Lamarckian theory completely collapses, and we must entirely abandon the principle on which alone Lamarck sought to explain the transformation of species,—a principle of which the application has been greatly restricted by Darwin in the discovery of natural selection, but which was still to a large extent retained by him. Even the apparently powerful factors in transformation—the use and disuse of organs, the results of practice or neglect—cannot now be regarded as possessing any direct transforming influence upon a species, and the same is true of all the other direct influences, such as nutrition, light, moisture, and that combination of different influences which we call climate. All these, with use and disuse, may perhaps produce great effects upon the body (soma) of the individual, but cannot produce any effect in the transformation of the species, simply because they can never reach the germ-cells from which the succeeding generation arises" (pp. 387–388). And much more in the same strain.

Weismann fully admits the influence of the environment upon the individual in producing marked changes. He also fully admits the facts of adaptation to environment and the transformation of species and development of organic beings. But he insists that natural selection is competent to explain all this, that it takes place through the selection of such accidental variations in the germ as prove advantageous, or, as he puts it, the selection from among an infinite number of ancestral germ-plasms in the fertilized ovum of such as will produce an individual most in harmony with its environment, leaving all others in the latent state. This, as we have seen, is pure Galtonism.

But this incapacity for the inheritance of acquired characters is confined to metazoans or multicellular organisms—organisms whose reproductive and somatic cells are differentiated. It does not apply to protozoans or unicellular organisms. These are greatly influenced by the environment, and, consisting entirely as it were of reproductive cells, naturally transmit their variations to their descendants directly. Only thus can variability be perpetuated, and whatever is true of them is true of all germ-cells. "The origin of hereditary individual variability," says Weismann, "cannot indeed be found in the higher organisms—the Metazoa and Metaphyta ; but is to be sought for in the lowest—the unicellular organisms. In these latter the distinction between body-cell and germ-cell does not exist. Such organisms are reproduced by division, and if, therefore, any one of them becomes changed in the course of its life by some external influence, and thus receives an individual character, the method of reproduction ensures that the acquired peculiarity will be transmitted to its descendants" (pp. 277-278).

It is here that comes in his fundamental doctrine of the continuity of the germ-plasm. If not the germ-cells, at least the germ-plasm of either parent passes intact to the offspring. It is perpetual, or as he calls it, immortal. It gives to the new being its special character, but receives nothing from it. It remains in the offspring until it in turn becomes a parent, and again passes to the third generation without ever having ceased to live. Every living being on the globe to-day contains in its germ-plasm something that has never ceased to live since the original life-breath was breathed into organic nature. Through all the ancestral types of the phyletic chain it has persisted, passing from parent to offspring through the transforming series, so that in the loins of the highest types of man there is something which was still living in the lowest primordial worm and even in the bathybian ooze of those primeval waters which in the earliest Cambrian times succeeded the formation of the original crust of the globe.

Upon this series of brilliant speculations and startling assertions, including much that it has been impossible for me to bring forward, has been founded the school of Neo-Darwinism. In Germany they attracted comparatively little attention, in France none, but in England they have become almost a shibboleth in the mouths of a large class of leading biologists.

It unfortunately requires something more than mere truth to arouse enthusiasm in many minds, and however much it may be disclaimed, it cannot probably be justly denied that the peculiar position of prominence and honor which this theory gives to the doctrine of natural selection, conceived and elaborated by Englishmen, had much to do with its especial charm for English ears. It is not to be supposed that Weismann deliberately bid for applause from England, but he could clearly see the tendency of his doctrines to exalt natural selection. He does not allude to this in any of his earlier essays, nor until he had begun to observe the effect his writings were producing in England. In the preface to his fifth essay, dated Nov. 22, 1885, however, he says: "The transmission or non-transmission of acquired characters must be of the highest importance for a theory of heredity, and therefore for the true appreciation of the causes which lead to the transformation of species. Any one who believes, as I do, that acquired characters are not transmitted, will be compelled to assume that the process of natural selection has had a far larger share in the transformation of species than has been as yet accorded to it; for if such characters are not transmitted the modifying influence of external circumstances in many cases remains restricted to the individual, and cannot have any part in producing transformation" (pp. 252-253). And in the last essay of this series, originally delivered in September, 1888, he further remarks: "But if the transmission of acquired characters is truly impossible our theory of evolution must undergo material changes. We must completely abandon the Lamarckian principle, while the principle of Darwin and Wallace, viz., natural selection, will gain an immensely increased importance" (p. 423).

A CRITIQUE OF WEISMANN.

I have now, as I believe, fairly if not fully stated, chiefly in the language of its founder, the Neo-Darwinian theory, and before passing to consider what has been said on the other side, and the position of the Neo-Lamarckians in general, I would like to pause a moment in order to offer a few reflections of my own upon Weismann's teachings. I am emboldened to do this the more not only because I have not seen the exact point of view from which they especially strike me touched upon by others in the voluminous discussion which has grown out of them, but also because what I shall say will be based entirely upon his own statement of the facts, and therefore the objection that, not being an embryologist, I am not competent to weigh the considerations from that side (which I would freely admit), cannot properly be raised.

The question is whether, accepting the continuity of the germ-plasm, accepting the nature which he ascribes to the fertilized ovum with its multitudes of ancestral plasms out of which selections are made, accepting his explanation of the meaning of the first and second polar bodies, accepting his differentiation into reproductive and somatic cells, and all the other details which he brings forward, many of which are, of course, only hypotheses, there do not still remain grounds on which to base a theory of the transmission of certain kinds of acquired characters, and especially those of a strictly functional nature. In fact, the question seems to me rather to be whether his line of argument carried to its extreme logical conclusion would not preclude the possibility of any variation whatever even in the germ-plasms themselves. It is not sufficient to say that all variation is due to the varied character of multitudinous germ-plasms in the fertilized ovum, brought there from many often remote ancestors possessing very different characters. This is a *petitio principii*, since it assumes these differences in those ancestors, and the primary question must be answered; whence these ancestral differences? How does he account for any differences at all?

We have already seen that Weismann restricts his denial to multicellular organisms and admits as a necessary part of his theory, that unicellular organisms are easily affected by the nature of their surroundings and activities, and that the changes thus produced are directly transmitted. "If for instance," he says, "a protozoan, by constantly struggling against the mechanical influence of currents in water, were to gain a somewhat denser and more resistent protoplasm, or were to acquire the power of adhering more strongly than the other individuals of its species, the peculiarity in question would be directly continued on into its two descendants, for the latter are at first nothing more than the two halves of the former. It therefore follows that every modification which appears in the course of its life, every individual character, however it may have arisen, must necessarily be directly transmitted to the two offspring of a unicellular organism (p. 278). . . . We are thus driven to the conclusion that the ultimate origin of hereditary individual differences lies in the direct action of external influences upon the organism" (p. 279). But he even goes further and asserts that there is no other way by which the germ can be affected. "I have never doubted" he says, "about the transmission of changes which depend upon an alteration in the germ-plasm of the reproductive cells, for I have always asserted that these changes, and these alone must be transmitted (p. 410) . . . In what other way could the transformation of species be produced, if changes in the germ-plasm cannot be transmitted? And how could the germ-plasm be changed except by the operation of external influences, using the words in their widest sense?" (p. 411).

Now if, as he insists, external influences cannot possibly affect the germs of metazoans, and if, as he here maintains, it is external influence alone that can influence any germs, it must follow that the only variation that could have taken place in the germ-plasms of the highest animals are those which occurred in the protozoan stage of their development.

This is clearly a *reductio ad absurdum*, derived entirely from his own statements, some of them among his latest utterances. The difficulty is to see why he should adhere so tenaciously to the idea that the germ-cells cannot be influenced by functional changes in the organism containing them. The mere fact that they are lodged within the body of an animal does not affect the question unless it can be shown that they are so lodged that no change is possible in the nature of their immediate surroundings. To assume this is gratuitous and contrary to what would be naturally supposed. In reading certain passages in his own book one is strongly tempted to doubt whether he believes it himself. For example, he says in one place speaking of hereditary variations: "I believe however that they can be referred to the various external influences to which the germ is exposed before the commencement of embryonic development. Hence we may fairly attribute to the adult organism influences which determine the phyletic development of its descendants. For the germ-cells are contained in the organism, and the external influences which affect them are intimately connected with the state of the organism in which they lie hid. . . It is even possible that the effects of these influences may be more specialized; that is to say, that they may act only upon certain parts of the germ-cells" (pp. 103–104). But he seems to see a great difference between this and the transmission of characters acquired in certain special organs to the same organs of the offspring. This would probably be clear only to an embryologist. One of the most suggestive thoughts in his whole philosophy is that of the total dissimilarity between the germ and the developed organism which is to result from it. He maintains with every semblance of truth that there can be nothing in common between them except the fact that the molecular structure of the germ is such that if allowed to develop it will produce a being similar to the one from which it sprung. This principle seems to be peculiarly applicable to the subtle influences which effect heredity, and without appealing to anything occult or abandoning the strictly casual and mechanical theory of heredity, it may be submitted whether we know enough about it as yet to assert that influences affecting the parental organism, even any of its organs, may not react specifically and in kind upon the germ and set up molecular tendencies in the same direction. This may be said quite independently of any attempt to explain precisely how it can do so, as the theory of pangenesis claims to do.

If the germ-plasms vary within the body of either parent before they are brought together that variation must be due to influences acting upon them in the animal body. All this Weismann admits, but he denies that the changes which he admits to take place in the individual as the result of changes in the environment and subsequent changes in the habits and activities of the creature can be regarded as among the causes which produce changes in the germ-plasm. Is this logical or even reasonable ? If not due to such changes to what causes are they due ? Without pretending to explain how such a thing could happen, I claim that the indications are that it does happen. To say without proof that it cannot happen adds nothing to the argument. We have an antecedent and we have a consequent. Both are facts. There is no possibility in the present state of our knowledge of either proving or disproving the casual connection between these facts. Variation takes place in the direction of adaption to changed conditions and activities. So far the inference is confirmed by a third fact. If the inference had not been challenged in the interest of another principle this would be regarded as proof. I do not agree with Weismann that the burden of proof rests on those who draw this natural inference. It rests on him and the Neo-Darwinians to show that the assumed cause is not a cause. This they have thus far failed to do.

You will understand that I am speaking of variations which take place in the germ-cells and sperm-cells of parental organisms before they blend in the fertilized ovum. Most of Weismann's argument is directed to show that the fertilized ovum itself cannot be affected by any transforming influence acting upon the mother during the growth of the embryo. This may be true but it is unimportant. The time required to develop the embryo is too short for the environment to produce any material change however strongly the tendency might be at the time in the direction of such change. It is chiefly the uncombined sexual elements which are admitted by all to be undergoing specific transformation. The Neo-Darwinians deny that this is due to admittedly parallel transformations going on in the individual, the result of external and internal influences upon the developed body ; the Neo-Lamarckians consider the latter as in great part the cause of the former, while admitting that other variations are taking place due to unknown causes and that these are seized upon by natural selection to the advantage of the species.

The difficulty, on Weismann's theory, of accounting for any variation at all above the protozoans still confronts us. If external influences can only act on unicellular organisms in such a way as to be transmitted, it must follow that so soon as the multicellular stage is reached a rigid fixity must result. One of these lower metazoans may undergo important modifications during its lifetime, but its offspring are always set back to precisely the same place where the parent was when it set out. All these functionally produced changes are, according to him, utterly lost because they cannot react upon the germ-plasm. Where is the room for the action of natural selection? He has not dwelt upon this point, but he would probably say, though contrary to statements above quoted, that the germ-plasms are constantly undergoing spontaneous variation and that natural selection works on these. We would then be brought back to where we were a moment ago, with the question still before us, how spontaneous variations differ from functional ones (for he would not maintain that they were wholly uncaused effects), and why it is not logical and rational to assume that functional changes are impressed upon the germ-cells in ways which, though unknown to us, are no more unknown than is the cause of spontaneous variations. This seems to be far more reasonable than the far-fetched, and, as it seems to me, childish view recently expressed by Prof. E. Ray Lankester, that the environment does indeed influence the germ-cells but only by kaleidoscopically shaking up their contents, thus causing what are called "sports" in the progeny, and that natural selection seizes upon these, thereby securing advantageous transformations.

NEO-DARWINISM.*

We will next briefly pass in review the extraordinary discussion which has followed chiefly from the publication of Weismann's essays. As already remarked, they produced very little influence upon the German mind, and most German investigators who noticed them at all, either saw little in them, or else attacked them with greater or less violence. It is almost exclusively in England that they have found favor, and here a veritable school of biologists has sprung into existence prepared to defend even the most extreme of Weismann's theories. It is due to the German investigator to say that, with the exception of the slight tendency above pointed out to dogmatize on the subject of the non-transmissibility of acquired characters, his essays are dignified and courteous and often evince an almost Darwinian modesty with regard to his own theories. Far different was the case with most of his English disciples. What he states as probable they assert as forever settled, and his working hypotheses become for them the fundamental truths of science. His papers were translated and reviewed, usually in an aggressive manner before any one had ventured to criticise them. Being usually beyond the reach of any but the embryological specialist all except ardent disciples reserved their judgment and declined to enter the field. At first there was an attempt to make it appear that Weismann's views reflected only those of Darwin himself and that all outside of them consisted in deviations and wanderings from his doctrines. It was sought to stamp them with the name of

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* The expression *Neo-Darwinian* was first used, so far as I am aware, by Dr. G. J. Romanes in a letter to *Nature* for Aug. 30, 1888 (Vol. XXVIII, p. 413), and occurs frequently in subsequent discussions. The substantive form *Neo-Darwinism* was a natural outgrowth from it.

"pure Darwinism,"* and the reader was frequently informed what Darwin really intended to say in certain passages which could not otherwise be made to harmonize with the new doctrine, and even in some still more refractory passages we are told what we would have said "if it had occurred to him." †

In default of any real opponent the Duke of Argyll, with his strong theological bias, his medieval spirit of logomachy, and his total lack of scientific ideas, was called out and set up as a sort of man of straw to be repeatedly demolished. But like the shadows in the valley of Walhalla, he emerged each time unscathed and renewed the deathless struggle. His presence in the arena had the further advantage for the new school of affording them an opportunity to point to him as a sample of the opponents of Weismann.

Against all this a few protests were raised from time to time and after the appearance of the English edition of the essays a few able and critical analyses were made. But the general character of the discussion as it has gone on in the columns of *Nature* and in the British magazines is such as I have described. The only other prominent or frequent contributor in answer to the disciples of Weismann is Dr. G. J. Romanes, and he has been more especially concerned with defending his priority to the idea which he has elaborated under the name of Physiological Selection, and to the discussion of certain phases of the law of *panmixia* which he claims to have discovered. It would, however, be unjust to deny that the discussion has been of value to science, since, had it done no more than to attract wide attention to so momentous a question it could not have been without its uses.

**Nature*, Vol. XXXVIII, Aug. 16, 1888, p. 364; Aug. 23, 1888, p. 388; Vol. XL, pp. 567, 619.

† See *Nature*, Vol. XLI, March 27, 1890, pp. 487, 488.

NEO-LAMARCKISM.*

Let us inquire what has really been done from first to last toward the demonstration, or scientific establishment of the law of transmission of functionally acquired characters and the preservation through heredity of the modifications produced by changes in the environment. It will not be necessary to go back to Lamarck as his presentation of the subject has been sufficiently dwelt upon. But I cannot agree with some recent writers that Lamarck was defending a totally different principle from that which is being defended to-day. It is true that Neo-Lamarckians recognize natural selection as an equally, and in some respects far more potent law, although, as has been justly insisted upon, it does not explain the cause of the variations of which it makes use. The Lamarckian principle does this, so far as it goes, and affords a true mechanical, that is, scientific explanation of the origin of species.

After Darwin himself, whose methods were always those of the true naturalist, unquestionably the most successful defender of this view is Mr. Herbert Spencer, whose methods are always those of the true philosopher. A man of such originality would be incapable of approaching the subject from the same standpoint as any of his predecessors, and we find him evolving this law from his great general scheme of mechanical cosmology, in which it appears as one of the equilibrating forces of the organic world. It is his law of "direct equilibration," natural selection forming a second law of "indirect equilibration."

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* Prof. A. S. Packard is believed to be the first to use the term *Neo-Lamarckian*. This he did in the introduction to the Standard Natural History (Vol. I, Boston, p. iii) in 1885, and on page iv he adopts the substantive form *Neo-Lamarckianism*. As the word *Lamarckism* had already been long in use the shorter form *Neo-Lamarckism* should be preferred.

Through the operation of these two principles the phenomena of adaptation are explained. Adaptation is placed by him in what seems to be a new light, as the tendency of the organism to respond through modification of form and structure to an ever-changing environment. The introduction of this form of words by Mr. Spencer has been of the utmost value to science in affording it a clear and precise terminology for the most important of all phenomena. Lamarck floundered about in straining after such a terminology. As I have shown he generally used the word *circumstances* for Spencer's *environment*, but in many cases he employed the word medium (*milieu*) and he occasionally approached the Spencerian expression so nearly as to speak of the environing medium (*milieu environnant*).* His idea was undoubtedly the same, but he lacked both the literary training and the philosophic power to present it in its best light.

Mr. Spencer showed that the general proposition that the organism must be permanently, constantly, and profoundly influenced by the environment is one that cannot be logically escaped. It is not a mere *a priori* deduction, but rests upon all the facts and phenomena of the organic world which he marshaled in a most masterly manner in its support. But the Neo-Darwinians who deny this because it conflicts with their new hypothesis, never cease to demand facts. Haeckel's reply to this was eminently just, that this new hypothesis is itself wholly unsupported by facts, in the sense in which they use the term. It is an inference from the study of embryology, and an opposite inference is as legitimate as the one they draw. The truth is that the real phenomena of heredity are too recondite for direct observation. We are dealing with the ultimate units of organic being and are compelled to judge of their actions by the general results. But Mr. Spencer went further than any one had done before him and brought together an immense array of the most convincing facts upon his side of the question. Although he wrote before the new hypothesis had been proposed he seems to have fairly anticipated it, and one is surprised to find the objections of the Neo-Darwinians clearly stated and squarely met. It would be needless to repeat his arguments here, even if there were time, but I may call attention especially to that which relates to the origin of those correlated structures which are necessary to render effective the modifications which natural selection or sexual selection has produced. He shows that unless these are due to inherited functional variations a series of violent assumptions must be made which put one's credulity to the severest test—not a pre-established harmony, but a multitude of pre-established harmonies, all of which must co-operate with unerring exactness. Under the hypothesis of the hereditary preservation of the functionally produced modifications necessary to secure these correlations the explanation is perfectly simple and rational. This argument, so far as I know, has never been answered, nor has any attempt been made to answer it.

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* Philosophie Zoologique, Vol. II, pp. 5, 304.

Early in the discussion of Weismann's theory and three years before the appearance of the English edition of his essays, Mr. Spencer seems to have foreseen their probable effect in England, and he turned aside from his systematic labors to reargue this question in the light of fresh facts and evidence. This he did in two articles in the Nineteenth Century for April and May 1886, which are characterized by an unfailing vigor of treatment and all the philosophic power which he is wont to display in the discussion of biological questions. I would especially commend the second of these articles as an altogether fresh presentation of the case, replete with facts from the lowest forms of organized life. Many of these taken from the vegetable kingdom come home to me with great force, and it seems difficult to see how another interpretation can be put upon them.

Prof. Karl Semper published in 1881 as one of the International Scientific series his Natural Conditions of Existence as they effect Animal Life, in which he supports the same class of views by many observations from his own profound studies. Prof. Sidney H. Vines in his Lectures on the Physiology of Plants (1886) offered some direct and telling strictures upon Weismann's teachings (Chap. XXIII), and after the English edition of the essays appeared he repeated these and answered categorically a large number of points in a communication to *Nature*.* Professor Weismann replied to this review, defending himself satisfactorily at some points, but was compelled to recede from several of his most important positions.

Mr. Patrick Geddes advanced in the Encyclopedia Britannica (Art. Variation and Selection) a somewhat novel theory of variation in plants, substantially in the same line, but probably with some vulnerable points, and Professor Henslow's recent work on the Origin of floral structures, seeks to show that "the responsive actions of the protoplasm in consequence of the irritations set up by weights, pressures, thrusts, tensions, etc., of insect visitors," have played the principal rôle in determining the forms of irregular flowers. In much of all this there is a tendency to extremism, and harm is often done by neglecting to recognize the action of natural selection where it is clearly present, but there always remains a residuum of facts which cannot be explained by that hypothesis.

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* Vol. XL, Oct. 24, 1889, p. 621.

Among those Germans who have so ably and systematically opposed the views of Weismann should doubtless first be mentioned Dr. G. H. Theodor Eimer. The work * in which he has most effectively undertaken this has been translated into English by Mr. J. T. Cunningham,† who is one of those who early took part in the discussion. The title of this work as well as the heads of some of the chapters (such as: "the influence of adaptation in the formation of species," "mental faculties as acquired and inherited characters," "evolution of the living world as the result of function," etc.) shows how directly Eimer antagonizes Weismann, and one of the leading merits of the book is the great number of new illustrations that it contains in support of his position.

Perhaps I should not pass over, in this imperfect survey, the able and very temperate paper of Mr. J. Arthur Thompson,‡ who, of all the writers here noted, comes the nearest to having anticipated the point of view of my own criticisms. The bibliography of the general subject which this writer gives at the end of his paper will enable any one who desires to pursue it further to supplement this brief enumeration to any extent, and also to take a retrospective view into its history and progress.

It would be easy to select from these and other works any required number of illustrations of the transmission of acquired characters, but there would not probably be one that Weismann would not find means of explaining away. He has

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* Die Entstehung der Arten auf Grund von Vererbung erworbener Eigenschaften nach den Gesetzen organischer Wachsens, Jena, 1888.

† Organic Evolution as the Result of the Inheritance of Acquired Characters, London, 1890.

‡ The History and Theory of Heredity. Proc. Roy. Soc. Edinburgh, Vol. XVI, 1888–'89, pp. 91–116. (Read Jan. 21, 1889).

taken up a number of such in his essays, stated them with sufficient fairness, and then proceeded to show that they are also capable of another interpretation. In some cases this is doubtless true, but in most cases his explanations seem strained and unnatural. In many they amount to an admission that the quality transmitted was functionally acquired and that the changed environment has actually influenced the germ. But he always insists that this does not constitute an acquired character. I do not see why it does not. For example, he says: "It is difficult to say whether the changed climate may not have first changed the germ, and if this were the case the accumulation of effects through the action of heredity would present no difficulty" (p. 98). I cannot see why this is not conceding the whole issue. Of course all modifications must first affect the germ, otherwise there could be no hereditary transmission. The only question is: Can the climate or the environment impress changes upon the germ? If yes, the Neo-Lamarckian asks no more. All that he contends for is conceded.

The quotation just made is from one of his earlier essays and he has objected to its being urged against him on the ground that it does not represent his latest conclusions. But what has he to say to the following from his eighth and last essay originally delivered in September, 1888?

"It is therefore possible to imagine that the modifying effects of external influences upon the germ-plasm may be gradual and may increase in the course of generations, so that visible changes in the body (*soma*) are not produced until the effects have reached a certain intensity" (p. 433).

It matters nothing to the Neo-Lamarckian whether the effects of external influences become visible in the first or the hundredth generation. The whole question is: Are they the cause of the modifications that actually take place? Weismann's English followers deny this and say that such modifications are due to the selection of accidental variations in the germ, and so in all cases. If the term "acquired" is to be any further refined away, then discussion is useless, for it is not a mere dispute about a word that interests us, but the fundamental question whether external conditions do or do not permanently and progressively influence the development of organic beings.

THE AMERICAN "SCHOOL."

Probably the strongest arguments that have been brought forward upon the affirmative side of this question are those derived from paleontology, and singularly enough, hitherto, so far as I am aware, this view of the question has been presented, with the single exception of Kowalevsky, entirely by Americans. This work was not done under the stimulus of Weismann's writings, because most of it was already accomplished before his essays appeared.

As far back as 1866 Prof. Alpheus Hyatt read a paper "On the Parallelism between the different stages of life in the Individual and those of the entire Group of the Molluscan order Tetrabranchiata," * in which were foreshadowed the views more definitely expressed in 1880 in his papers "Upon the Effects of Gravity on the forms of shells and animals," † and "The Genesis of the Tertiary Species of Planorbis at Steinheim." ‡ In these papers Professor Hyatt showed the moulding influence of what in this case happened to be an

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* Mem. Bost. Soc. Nat. Hist., Vol. I, p. 193. (Read Feb. 21, 1866).

† Proc. A. A. A. S., 1880, p. 527.

‡ Mem. Bost. Soc. Nat. Hist., Fiftieth Anniversary, 1880. Second Memoir.

environment growing gradually less and less favorable, but not the less adapted to display in a very clear light some of the most important laws of transformation.

In 1877 Mr. John A. Ryder read a paper "On the laws of digital reduction,"* showing the obvious adaptations to the changing environment which had taken place in vertebrates in this respect, and a year later he pursued the same line of argument for modifications of the teeth.†

Mr. Ryder has for many years past been engaged in embryological researches, but there is no evidence that they have led him to abandon the views expressed in these earlier papers in favor of those of Weismann. On the contrary, several comparatively recent papers of his ‡ consist in great part of direct attacks upon Weismann's teachings and criticisms of his embryological theories.

Professor Cope commenced publishing on this subject at about the same time and has continued to study the vertebrate fauna of America without interruption to the present time. It appeared to him from the first that paleontology affords proof of the causes of variation, as revealed in the wonderfully complete transition series that are found in the teeth, toes, and various parts of the skeleton of extinct animals, adapting them to a changing environment and higher structural perfection. The study of living animals cannot, in the nature of things afford any such series of forms, and the evidence from paleontology is particularly striking in this respect. A volume of Professor Cope's memoirs was published in 1887 under the title of "The Origin of the Fittest," by which title he aimed to express the idea of the cause or origin of modifications that have taken place, as distinguished from Darwin's explanation of the laws of transformation based on the assumption of such modifications taken as simple facts of observation. Both methods are scientific, but the former carries us one step nearer to the true origin of things.

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*American Naturalist, Vol. XI, October, 1877, pp. 603–607.

†On the mechanical genesis of tooth forms, by John A. Ryder. Proc. Nat. Sci. Phil., Vol. XXX, 1878, p. 45; Vol. XXXI, 1879, p. 47.

‡The Origin of Sex through Cumulative Integration and the Relation of Sexuality to the Genesis of Species. Proc. Am. Phil. Soc., Vol. XXVIII, May 29, 1890, pp. 109–159.

A Physiological Hypothesis of Heredity and Variation. Am. Nat., Vol. XXIV, January, 1890, pp. 85–92.

More recently Prof. H. F. Osborn of Princeton College has taken up this line of argument and presented it in several memoirs in which he has attempted a direct answer to Weismann's charge that no facts have been furnished in support of the transmission of acquired characters.*

In the latest of these papers, that read before the Society of Naturalists in Boston December 31, 1890, not yet published, but of which an advance copy was kindly sent me by him, he has stated the whole problem with a judicial fairness which all must admire, and with a keenness of analysis which places him in the front rank of modern biological thinkers.

Perhaps the most important contribution which he has made to the subject is that in which he shows that "the main trend of variation is determined not by the transmission of the full adaptive modifications themselves, as Lamarck supposed, but of the disposition to adaptive atrophy or hypertrophy at certain points."

This principle goes farther than any other that has been brought forward to differentiate Neo-Lamarckism from Lamarckism proper, while at the same time it is an effective answer to a large part of the argument directed against the transmission of functionally acquired characters.

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* Proceedings of the American Association for the Advancement of Science, Vol. XXXVIII, 1889 (Toronto), pp. 273–276.

British Association Report, 1889 (Newcastle-upon-Tyne), p. 621; Nature, Vol. XLI, Jan. 9, 1890, p. 227.

American Naturalist, Vol. XXIII, July, 1889, pp. 561–566.

Professor Osborn has probably made the most of the argument from paleontology, and it must be left to the candid judgment of scientific men to say whether the case is made out. It is of course always possible to say that the initial variations which inaugurated each new adaptation were merely accidental and were seized upon by natural selection, and it is to a large extent a question of faith in the universal efficacy of that theory ; or rather a question in candid minds of the relative reasonableness of that view and of the view which ascribes a considerable part of this initial variation to functionally produced modifications transmitted by heredity.

It would be unjust to this Society to omit in an enumeration, however imperfect, of the American defenders of the transmissibility of acquired modifications, your former president Prof. W. H. Dall, whose protracted studies in invertebrate paleontology, conchology, and especially the molluscan life of the deep sea have led him to a full accord with other American workers as regards questions of this class. In his presidential addresses, not to speak of earlier papers, he has emphasized the molding influence of the environment upon the plastic organisms with which he is most familiar, and during the past year he has contributed to the Society one paper* dealing directly with the Neo-Darwinian claims, in which the case is as clearly presented as it has been by any other writer, and in many respects in an entirely new light.

For myself, I cannot claim to have made any direct contribution to this specific subject. I have been deeply interested in the development of plant life and have from time to time within the past fifteen years presented this theme from every point of view that I have been able to see it. I recognize the law of natural selection as probably the most potent of all organic laws, but I have never doubted that a great part of the variations upon which its action depends are due to reactions of the organism upon the environment, and after reading Weismann's essays and every scrap of discussion that I have been able to find arising from them, I am still so dull as to remain unconvinced that such modifications are incapable of hereditary transmission. To say that the environment may and must influence the germ, but that it can only influence it in a hap-hazard way analogous to that in which a jar affects the figures of a kaleidoscope, is to my mind a begging of the question, and I prefer to assume that there is a causal connection between the nature of the influence on the germ and the alterations that result, especially as the latter are admitted to be in harmony with the former.

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*On Dynamic Influences in Evolution, by W. H. Dall. Proc. Biol. Soc. Wash., Vol. VI, pp. 1–10.

If I have succeeded in showing in one of my papers before this Society* that considerable variation is constantly taking place irrespective of any advantage to the species, this much at least has been withdrawn from the domain of natural selection, and if these changes are not produced by that law there seems no escape from the conclusion that they are caused by some unknown external influences.

In the foregoing review of the work that has been done toward the scientific demonstration of the transmissibility of functionally acquired characters I do not pretend to have given the arguments themselves. I have only pointed out the fact that they have been presented, by whom, from what branch of science, and under what circumstances, and I must leave it to each of you, if sufficiently interested, to study them for yourselves from the original sources.

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*Fortuitous Variation as illustrated by the genus Eupatorium. Abstract in Nature (London) Vol. XLI, July 25, 1889, p. 310.

APPLICATION TO THE HUMAN RACE.

The wide-spread agitation of a problem of this nature, however technical or recondite it may be, lying as it does on the very ocean bed of science, cannot help sooner or later making itself felt at the surface and producing its normal influence upon the great practical questions of the moral and social world. And the nature of this influence, fortunately for us, is some indication of the truth or falsity of the views defended. Just as the mathematician knows, when his calculations lead him to just and rational results that his assumption was a true one, and when they lead to a series of negations and absurdities, that it was a false one, so we may expect that if the assumption of the non-transmissibility of acquired characters is a sound one the practical conclusions that flow from it bearing upon the affairs of life will harmonize with the best thought on the development of the human race ; and conversely, if its application to practical life conflicts with such best thought and with the facts of history and of social progress we are justified in the inference that it is an unwarranted assumption. What do we find ?

The highly artificial character of what we call civilization is a fact which I have for many years sought to enforce by a variety of illustrations. That nothing like it could ever result from the natural flow of the forces that have combined to produce it is too obvious to require explanation, and that human advancement in general is exclusively the result of the exercise of man's intellectual power in the artificial direction of the raw forces of nature into channels of human advantage, is a proposition which only needs to be understood to be universally admitted. The tendency of the scientific mind to apply to social phenomena the canons that prevail in the non-intelligent world, is at least as ancient as the French physiocrats, Adam Smith, Ricardo, and Malthus, and it has been strengthened since Darwin by the writings of some of the ablest social philosophers. It rests on the seductive idea that what nature does must be well done, and that nature's methods must be the best methods for man to adopt. I have hitherto designated this kind of philosophy as a sort of nature-worship, and shown that the entire fabric of reasoning crumbles away at the first touch of critical analysis. But it is a fascinating habit of thought and difficult to dislodge from a certain type of mind.

Now on examining the practical applications which the Neo-Darwinians make of their underlying conception, I find them to be strikingly in line with those last described. If nothing that the individual gains by the most heroic or the most assiduous effort can by any possibility be handed on to posterity, the incentive to effort is in great part removed. If all the labor bestowed upon the youth of the race to secure a perfect physical and intellectual development dies with the individual to whom it is imparted why this labor? If, as Mr. Galton puts it, nurture is nothing and nature is everything, why not abandon nurture and leave the race wholly to nature? In fact the whole burden of the Neo-Darwinian song is: Cease to educate, it is mere temporizing with the deeper and unchangeable forces of nature. And we are thrown back upon the theories of Rousseau, who would abandon the race entirely to the feral influences of nature.

The great men who talk this way, trained in the methods of the university, their minds stored with the fundamental, comprehensive, and organized materials for thinking and working which modern methods of education could alone have given them, use these materials, and take advantage of this training to spin out a subtle thread of reasoning which results in condemning the only means by which they were enabled to comprehend questions of this nature. Professor Weismann could never have prosecuted those prolonged investigations which have given him such a grasp of the intricate problem of heredity had he not been trained in the rigid methods of the German universities. Nay, those rigid methods themselves have been the product of a series of generations of such training, transmitted in small increments and diffused in increasing effectiveness to the whole German people. It has not been brought about by natural selection which only selects such ancestral germ-plasms as increase the certainty of reproduction. Such habits of mind could have no such tendency. They secure no advantage in the struggle for existence. And the fact that out of the barbaric German hordes of the Middle Ages there has been developed the great modern race of German specialists is one of the most convincing proofs of the transmission of acquired characters, as well as of the far-reaching value to the future development of the race of such an educational system as that which Germany has had for the last two or three centuries.

It was said of Mr. Darwin that he was himself a good illustration of the law of atavism which he formulated since his habit of mind lay latent in his father and came to him from his grandfather Erasmus. Similarly it might be said that Professor Weismann is as good an example as need be asked of the transmission of acquired characters and of the hereditary embodiment of that wide-spread German characteristic which has been the increasing product of the German educational system and of German institutions.

Mr. Herbert Spencer has followed out this same line of reasoning as applied to the great development of the musical faculty in Germany, and shown that the Haydns, Mozarts, Beethovens, the Liszts, Rubensteins, and Wagners, have formed, as it were, the several peaks of a great hereditary musical uplift in the German nation. The same is true of Italy, not only in music, but especially in sculpture, and we have there, so to speak, a race of sculptors. Those who, without any patriotic bias, compared the Italian and American pieces at the Centennial Exhibition could not help being impressed with this. There could be seen the most exquisite pieces of statuary, in which not only features of rare perfection and beauty, but every form of drapery were represented in marble with a trueness to life that almost deceived the looker on. And to such pieces were attached, not one or two celebrated names, but a great number of names of artists unknown to the public outside of those who make sculpture a special study. Contrasting these perfect productions with the lifeless ones that represented the highest reaches of American sculpture, even those produced by Americans who had spent many years at Rome and worked in an atmosphere of Italian sculpture, I was impressed with the little that a single generation can accomplish in such things, and with the fact that in Italy we have a race of born sculptors who inherit their deftness from ancestors as remote as Michel Angelo.

Weismann has not ignored the arguments from this side, but his attempts to meet them are among the weakest of all his reasonings. Here are some samples of them : "The children," he says, "of accomplished pianists do not inherit the art of playing the piano ; they have to learn it in the same laborious manner as that by which their parents acquired it ; they do not inherit anything except that which their parents also possessed when children, viz., manual dexterity and a good ear" (p. 269). "The pianist . . . may by practice develop the muscles of his fingers so as to ensure the highest dexterity and power; but such an effort would be entirely transient, for it depends upon a modification of local nutrition which would be unable to cause any change in the molecular structure of the germ-cells, and could not therefore produce any effect upon the offspring" (p. 278). If this were true nothing is more certain than that the talent for piano execution could be no higher in the ten thousandth generation than that attained during the first, and that the curve representing the progress of music, sculpture, the talent for special scientific research, or any other form of genius, would be an irregular line with absolute average horizontality instead of what we know it to be in every case, an irregular, but progressively ascending curve marking a great forward movement.

It is universally conceded that the evidence for the transmission of acquired mental qualities is much stronger than for those of any other class, chiefly because they are entirely withdrawn from the action of natural selection, not tending in the least to the survival of the fittest. It has therefore been necessary for Weismann to deny their transmission at all. This is so palpably contrary to the facts of human history that few have been willing to follow him to this length. It is well known that Mr. Wallace has always excepted the human race from the action of natural selection, but in so doing he has seen fit to abandon the scientific method entirely, and in his last work he makes a complete break in the continuity of development with the advent of the higher psychic faculties, calling in an independent spiritual attribute to account for this class of phenomena. Prof. E. Ray Lankester, the foremost of Neo-Darwinians, in reviewing this work of Mr.

Wallace[*] makes the following remarks on this point: "Mr. Wallace's contention that the mathematical and artistic faculties of man have not been developed under the law of natural selection must in large part be conceded . . . . their sudden and rapid development to a very much higher level in civilized communities cannot be traced to the struggle between man and man. It does not however follow that, because natural selection will not account for these extraordinary developments of the human brain, therefore we must have recourse to the assumption of supernatural agencies. Mr. Wallace seems so much convinced of the capability of the principle of natural selection, that when it breaks down as an explanation he loses faith in all natural cause, and has recourse to a metaphysical assumption." But Prof. Ray Lankester, estopped by his consistent defense of Weismann's views, is obliged to ignore the obvious explanation that the intense exercise of these faculties, impressing itself profoundly upon the plastic brain substance and reacting upon the germs of posterity has been transmitted to descendants through centuries of developing civilization, and he has recourse to his doctrine of "sports" and to Gulick's law of "divergent evolution" which is nearly the same as what I have called "fortuitous variation."

But we need not confine ourselves exclusively to the mental qualities. A favorite illustration of the efficacy of selection is the progress which has been secured in the fleetness and other desired qualities in horses, and Mr. Wallace, in the Fortnightly Review for September 1890, has instituted a contrast between what would result in this direction from a system of intelligent breeding and one of mere feeding and exercise. His illustration is thoroughly unfair, even ridiculous, since he does not attempt to transmit the acquired superiority but allows it to be diluted and lost by promiscuous breeding with stock that has not been subjected to any training. As a matter of fact training enters largely into the development of superior breeds of horses, and great care is taken that educated strains be bred together. And breeders as a rule would ridicule the idea that all their training goes for nothing, and that it is only accidental variations that can be bred into the new race of horses.

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[*] Nature, Vol. XL, Oct. 10, 1889, pp. 569-570.

But let us take another case in which natural selection is wholly excluded. It is well known that a steady and uniform progress has been going on for a century or more in all forms of gymnastic skill and feats of bodily suppleness by men constantly in training for the purpose, which is comparable to that which has taken place in the trotting power of horses. Every year new wonders are brought before the public and the feats of the previous year are exceeded by some fresh virtuoso. This is accomplished, I am told, by lifelong training of the children of acrobats and of their children, thus producing, as it were, a little race of acrobats. What care is taken to prevent the loss of much of this through marriage outside of the trained stock, I do not know, but certain it is that great progress in physical development has taken place and is taking place, and there is no doubt whatever that it is largely due to the transmission of the qualities directly acquired by training.

In fact, Mr. Galton's conclusions, notwithstanding his doubts about the transmission of acquired talents, are not only not opposed to that view but in great part confirmatory of it. He is led by a carefully conducted series of inquiries and investigations to believe that genius is in the main hereditary; that the exceptionally talented and highly endowed are descended from talented and highly endowed parents, etc. But this only throws the question back one generation farther, and it remains to be shown that such talent and endowment in their ancestors was not the result of education, personal effort, or some other form of acquirement and not of mere accident.

But the great debate on heridity seems destined to secure still other and more far-reaching advantages. Not only has it assured us that we may hand our good works down to posterity through the law of the transmissibility of acquired qualities, but it may and should teach us that the all-powerful law of selection is also an instrument in the hands of intelligence for the working out of human destiny. It is the right and the duty of an energetic and virile race of men to seize upon every great principle that can be made subservient to its true advancement, and undeterred by any false ideas of its sanctity or inviolability, fearlessly to apply it. Natural selection is the chief agent in the transformation of species and the evolution of life. Artificial selection has given to man the most that he possesses of value in the organic products of the earth. May not men and women be selected as well as sheep and horses? From the great stirp of humanity with all its multiplied ancestral plasms—some very poor, some mediocre, some merely indifferent, a goodly number ranging from middling to fair, only a comparative few very good, with an occasional crystal of the first water—from all this, why may we not learn to select on some broad and comprehensive plan with a view to a general building up and rounding out of the race of human beings? At least we should by a rigid selection stamp out of the future all the wholly unworthy elements. Public sentiment should be created in this direction, and when the day comes that society shall be as profoundly shocked at the crime of perpetuating the least taint of hereditary disease, insanity, or other serious defect as it now is at the comparatively harmless crime of insect, the way to practical and successful stirpiculture will have already been found.

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